EXPERIMENTS
ON THE
RED AND QUILL
PERUVIAN BARK:
WITH
OBSERVATIONS
ON ITS
HISTORY, MODE OF OPERATION, AND USES;

And on ſome other Subjects connected with the

PHÆNOMENA AND DOCTRINES OF VEGETABLE ASTRINGENTS.

BEING A DISSERTATION
Which gained the FIRST PRIZE given by the HARVEIAN
SOCIETY of Edinburgh for the year 1784.

BY RALPH IRVING.

> Omnes enim trahimur et ducimur ad cognitionis et ſcientiæ cupiditatem.
>
> CICERO de Officiis.

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DISSERTATION

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BY

HIS MUCH OBLIGED HUMBLE SERVANT,

Edinburgh,
Dec. 16. 1784.

RALPH IRVING.

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INTRODUCTION.

AFTER the very valuable publications of Doctors M'Bride, Percival, Saunders, and other eminent men, any farther examination into the nature and properties of the Peruvian bark may to ſome appear unneceſſary.

I do not however find, that the general ardour of Phyſicians on this ſubject is at all abated.

If indeed we conſider the endleſs ſucceſſion of new objects and difficulties which ever ſtrike a traveller in an important path of experimental philoſophy, we ſhall ſoon be ſatisfied, that each fortunate ſolution of a doubt ſeldom fails to create a new one.

"Hills peep o'er hills, and Alps on
Alps ariſe."

The inquiry before us is diverſified and extenſive, as being connected with many uſeful doctrines in pharmacy, and even in the chemical philoſophy.

To point out its importance in the practice of phyſic, would be an inſult on the underſtanding of all medical men.

Every intelligent Phyſician likewiſe knows, that to handle the ſubject with any degree of preciſion, muſt require a conſiderable knowledge of, and may throw light upon, ſome of the moſt important doctrines of the animal œconomy.

When my friends are thus acquainted with my own ſentiments on the nature of the buſineſs I attempt, they cannot poſſibly be ſurpriſed that I ſhould have ſo long heſitated to hold out this Eſſay to the public view.

Though theſe Experiments have been long under my eye, and many of them frequently repeated, I muſt ſtill think them in a very crude imperfect ſtate.

As ſuch I preſent them, with all the diffidence natural to a very young Author on his firſt performance.

I hope, however, that thoſe diſpoſed to do me the honour of examining them, will do it with candour, and "that they will not condemn my opinions, till they ſhall be certain that they fully comprehend my meaning, and ſhall be acquainted with the proofs I can produce to confirm them." If ſuch ſhould fail me, I am ſtill able to reſt undiſturbed, being taught to believe, that "he who does not fooliſhly affect to be above the failings of humanity, cannot be mortified when it is proved that he is but a man."

As theſe Experiments were begun, and moſtly carried on, during my reſidence in an obſcure country village, far remote from the aſſiſtance of men or books, it cannot be expected that I ſhould be able to furniſh any very full hiſtory of the Peruvian bark; a taſk, indeed, which might afford ſufficient matter for a ſeparate diſſertation.

But as my inquiries led me into paths rather uncommon and little frequented, ſome general account of the introduction and progreſs of this celebrated remedy, may perhaps ſatisfy the curioſity of thoſe who think it unneceſſary to explore in the ſame circle.

We ſhall find, that Cinchona, the name of this tree, adopted by Linnæus, took its riſe from a very particular ſtory, to be hereafter mentioned.

Linnæus places it among the *Pentandria monogynia*, and defines two ſpecies.

The *Cinchona officinalis-panicula brachiata* is that deſcribed by Ray, and is the tree which furniſhes our Peruvian bark.

The ſecond is the *Cinchona pedunculis unifloris*. It is a native of the Caribbee Iſlands; and were we to regard the ſtrict propriety of names, it is perhaps improperly called *Cinchona*, as it does not appear that this ſpecies was known, or at leaſt uſed, in the days of the Viceroy Cinchon.

This ſpecies is the Cinchona Jamaicenſis of Dr Wright, Phil. Tranſ. vol. lxxvi. part 2. It is the *liber* of this tree that poſſeſſes the properties of the Peruvian bark.

The Cinchona Caribbæa has a taſte reſembling that of horſe-radiſh, with a mixture of the eaſtern aromatics. From ſome experiments it is found that it yields more of its extractive matter to water than the Cinchona of Peru. Some additional account of this bark was communicated by Dr Wright in a letter to Dr Duncan, publiſhed in the fifth volume of the Medical Commentaries. Dr Wright found it very uſeful in the remittent fevers of the Weſt Indies, and in nervous fevers. It cured an intermittent fever in London as effectually as the Peruvian bark.

I am informed by my friend Mr Telford of the Navy, that in St Lucia was diſcovered, by Dr Young, late Phyſician to the Army Hoſpital of that place, a tree which he thought reſembled the Cinchona officinalis of Linnæus. The Doctor, therefore, tried ſome of the freſh bark on obſtinate intermittents in the hoſpital. He found that it operated very violently as an emetic and cathartic; but cured the intermittents even after the Peruvian bark and other remedies had failed. By drying, it loſt its emetic and cathartic effects, but ſtill cured intermittents. The Doctor put ſome of this bark into the hands of Dr Blain, Phyſician to the Fleet, Mr Telford, and ſome other ſurgeons of the Navy, with whom its ſucceſs was nearly equal to that of Dr Young's.

Some of this bark has been lately imported into Britain, and was tried by Dr Henry Cullen of the Royal Infirmary of this place, but not ſeemingly with the ſame ſucceſs as experienced by the gentlemen above mentioned. It may probably prove an uſeful ſubſtitute for the Cinchona of Peru, if our ſupplies from that country ſhould fail.

It is only however of the firſt, or Cinchona officinalis, that we mean to ſpeak in this place.

We are told, that this tree is ſeldom more than two toiſes and a half high; that it grows in foreſts promiſcuouſly, and is ſpontaneouſly propagated from its ſeeds falling on the ground.

Theſe trees grew formerly in great plenty on the hills near the city Loxa, or Loja, in the province of Quito in Peru.

They were however found in other places beſides theſe mountains.

Thoſe moſt eſteemed grew on the mountain of Cajanuma, two leagues to the ſouth of Loxa.

They have alſo been diſcovered in the

neighbourhood of Riobambo, Cuema, and ſome other countries in the province of Quito.

Lemery and Pomet alſo tell us, that another kind of theſe trees grows on the mountains of Potoſi, and that they are the beſt, but much ſcarcer than any of the reſt.

"The tree," ſays Pomet, "is about the ſize of a cherry-tree; the leaves round and indented; it bears a long reddiſh flower, from whence ariſes a kind of pod, in which is found a kernel like an almond, flat and white, clothed with a ſlight rind." Bolli compares the leaves to thoſe of the pruni rubæi; and ſays, they impreſs an aromatic and ſomewhat bitter taſte upon the tongue; that the tree is adorned with white and azure-coloured flowers, like thoſe of the pomegranate, but bears no fruit.

"The bark," ſays Pomet, "which comes from the trees on the bottom of the mountains, is thicker, is ſmooth, of a whitiſh yellow without, and of a pale brown within: That which comes from trees on the top of the mountains is abundantly more delicate; it is uneven, browner without, and of a higher colour within: But the trees which grow on the middle of the mountains, have a bark yet browner than the other, and more rugged. All theſe barks are bitter; but that from the trees on the bottom of the mountains leſs ſo than the others." He therefore prefers the bark of thoſe trees growing on the middle of the mountains.

As Bolli tells us, that the leaves are aromatic, and ſomewhat bitter, it is probable that they poſſeſs ſome medicinal quality. The wood and flowers are ſaid to be quite inſipid. The reſin, however, that exſudes from the wood, but eſpecially from the bark, and likewiſe the ſeeds of the tree, though much inferior to the bark, are ſaid to have ſimilar effects.

It has been generally ſuppoſed, that the Indians were acquainted with the uſe of this bark ſo early as the year 1500. It has alſo been alledged, that the diſcovery was owing to the accident of a diſeaſed Indian, drinking from neceſſity of ſome ſtagnant water, wherein this tree had been long macerated.

Theſe, however, like all oral traditions of a rude people, muſt be very ambiguous, and are ſeldom of much importance in our inquiries. The natives are ſaid to have known it by the names of *Ganapride*, *Guananepide*, *Chinanepide*, and *Guananegine*.

We are not ſufficiently acquainted with the language of thoſe countries to underſtand the import of theſe names. It is however probable, that ſome of them are derived from the known virtues of the remedy, and others from particular circumſtances reſpecting its diſcovery and appearances.

Every thing ſingularly uſeful to mankind

kind has, at different periods, acquired different appellations, according to the grateful enthuſiaſm of the vulgar, the learning of the hiſtorian, or the fancy of the bard.

It is therefore probable, that the bark had been long and much eſteemed in Peru, before its introduction into Europe.

Their method of uſing it was very ſimple and efficacious, and was perhaps ſuggeſted to them from the manner in which it was diſcovered. Unacquainted with the art of the mortar, they infuſed it a whole day in water; and, unfettered by theory, they gave it in ſuch doſes as experience taught them were ſufficient to overcome the diſeaſe. It hence appears, that the firſt method of uſing the bark was in the form of the cold infuſion; and we ſhall afterwards find, that the effects of this preparation were ſufficient to juſtify the eſteem of the inhabitants of Peru.

A very extraordinary blank now interpoſes in our hiſtory of the Peruvian bark.

We find *Pizarro*, ſo early as the year 1530, landing a powerful armament on the frontiers of Peru, and, after no long time, penetrating into the heart of the country.

Amongſt numberleſs diſcoveries of their cuſtoms and manners, and particularly of their wealth, we hear no account of the *Ganapride* of thoſe harmleſs men, till about the middle of the ſeventeenth century.

Such diſcoveries were indeed poor objects for a rapacious and illiterate army, whoſe every path was marked with cruelty and ſlaughter.

Should we have expected more attention from the miſſionaries and other prieſts, the anſwer is, their meditations were more engaged in the mines of Potoſi, than in the foreſts of Loxa.

There are perhaps few circumſtances

in the hiſtory of that black ſcene, which more clearly prove the bitter and determined hatred of the inhabitants of the New World to their European conquerors.

Nor can we wonder, that thoſe unlettered men denied one of their greateſt bleſſings to beings whom they eſteemed the tyrants of the world.

It is however remarkable, that what they ſo carefully concealed from their enemies, they at length loſt ſight of themſelves.

If we reflect but a moment on the calamities and diſaſters which overſpread their country, we can be at no loſs to find an explanation for this circumſtance. Nor is it probable, that, even after the rigour of the Spaniſh cruelty was abated, the natives could acquire the knowledge of a remedy, uſed by anceſtors either fallen by the ſword, or loſt to every ſenſe of enjoyment.

The Abbé Raynal tells us, that they

had ſo far forgot their Ganapride, that they imagined it was employed in Europe for dyeing, till Juſſieu, a French botaniſt, taught them its uſe in intermittents, about thirty years ago.

The ſame author aſſerts, that it was known in Rome in the 1639; and that, in the year following, John de Vigo, phyſician to a Vice Queen in Peru, eſtabliſhed it in Spain at an hundred crowns a pound.

The Abbé Raynal has, however, made the date of its introduction into Europe ſome years prior to that generally fixed on by other authors.

The firſt authentic information of its virtues, received by the Europeans, ſeems to have originated from the following well atteſted circumſtances.

In the city of Lima, the metropolis of Peru, the Comitiſſa or Lady of *Count del Cinchon*, Viceroy of Peru, was ſeized with a tertian fever. The rumour was ſoon ſpread far and wide, and after no long time reached the city Loxa. In a letter from a celebrated phyſician at Loxa, (who, I ſuppoſe, was the John de Vigo of the Abbé Raynal), the Viceroy was earneſtly adviſed to give his Lady this bark; which being complied with, the Vice Queen immediately recovered. Hence the bark derived the names of *Cinchona*, *Chinchina*, *China China*, *Kinkina*, *Quinquina*, *Pulvis Comitiſſæ*, *&c.*

By this means the reputation of the bark was firmly eſtabliſhed in Peru; and the Viceroy ſoon after leaving India, ſpread the fame of the new febrifuge over Old Spain.

About the year 1649, its celebrity was ſtill increaſing, and not only in Spain, but alſo in Italy, eſpecially in Rome. This was chiefly owing, partly to the pious induſtry, and partly to the avarice of the Jeſuits, and particularly of the Cardinal de Lugo, then Spaniſh Viceroy in the Weſt Indies. Hence the bark derived the names of *Pulvis Patrum*,

trum, *Pulvis Jeſuiticus*, *Pulvis Cardinalis de Lugo*, and in our own language, the *Jeſuit's bark*.

Its reputation now made rapid progreſs over all Europe. In the year 1663, was publiſhed at Genoa, the *Anaſtaſis Corticis Peruviæ*, by *Sebiaſtinus Badus*. Amongſt much political and medical hiſtory of this bark, that author would have us to believe, that it was known, and in a great meaſure eſtabliſhed in other parts of Europe, when it was ſhamefully rejected in England *.

Allow me here to produce the authority of as faithful an obſerver as Genoa ever ſaw. Dr Sydenham, in his firſt *Epiſtola Reſponſoria* to Dr *Brady*, tells us, that, to the beſt of his remembrance, the bark was celebrated in London about twenty-five twenty-five years ago. Now, if we ſuppoſe, as indeed we muſt, that this epiſtle was wrote not long after Dr Brady's letter, which is dated in the year 1679, we ſhall find, that the bark firſt acquired reputation in London ſo early as the year 1654.

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* Quippe fama mendaci potius quam propria experientia inſtructi, ſaluberrimam hanc medicinam reſpuebant, quum tot alii medici in omni Europa, ſive in Gallia, ſive Hiſpania, Germania, Flandria, Italia, eam nedum uſurpant, quin et miris laudibus extollunt.

Fraſſoni was undoubtedly among the firſt who uſed bark on its introduction into Italy; and his letter to *Badus*, inſerted in that author's *Anaſtaſis*, is dated in 1656, or two years poſterior to its celebrity in London. It does not appear that the bark was even then eſtabliſhed in Italy; for Torti tells us, that ſuch was the antipathy of phyſicians againſt it, that, till the year 1680, (when Fraſſoni died), the bark, to eſcape the ſevere tribunal of phyſicians, durſt not enter the ſhops, but was kept as it were privately amongſt the cloiſters of the nuns. Even as late as the year 1714, we find *Ramazzini* oppoſing it with fanatic acrimony; and, after his death, the conteſt maintained by *Mangetus*, *Baglivi*, and many others. Torti, in the year 1719, wrote his *Mutinenſium Medicorum Methodus antipyretica vindicata, &c.* after which, the phyſician who writes the account of his life, tells us, in words highly expreſſive of the indiſcreet fury of the diſputants, "Nullus deinde hiſcere eſt auſus." It therefore appears, that the bark was neither much earlier nor more cordially received in Italy than in England.

The uſe of the bark in France ſeems to have been in a great meaſure deſiſted from, till our adventurous countryman, ſo early as the year 1679, by a bold, though fortunate practice, reſtored its fame and honour. As he was cotemporary with Sydenham, there is every reaſon to think that he learned his *Engliſh remedy*, or *wonderful ſecret*, from the practice or writings of the Engliſh Hippocrates. I am therefore diſpoſed to think that Dr Sydenham was the true reſtorer of the uſe of the Peruvian bark, and in ſuch doſes as infallibly eſtabliſhed its reputation *. Talbot's enterpriſing activity well merited his fortune and his knighthood.

Whether the tree furniſhing the red † bark

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* When I ſpeak of the reputation of the bark being eſtabliſhed, I mean with reſpect to the opinion and practice of the leading phyſicians of the age. It is well known, that, at that time, and even to the preſent day, many of the inferior phyſicians, and ſome of the vulgar, have uſed the bark as a ſubject wherewith to humour their exclamatory ſmatterings. Theſe, however, may be ſafely paſſed over, as no more affecting our general hiſtory than influencing the opinions of candid poſterity. When the juſtly celebrated Mr Dryden wrote the following lines, he was addreſſing himſelf to a bachelor, who, he knew, was, or affected to be, equally averſe to a phyſician and a wife.

"The tree of knowledge firſt in Eden plac'd,
Was eaſy found, but was forbid the taſte.
Oh! had our grandſire walk'd without his wife,
He ſoon had ſought the better plant of life.
Now both are loſt; yet, wandering in the dark,
Phyſicians, for the tree, have found the *bark*."

† We are informed, that, in the province of Santa-Fe, thirty degrees north of the equator, two kinds of the Cinchona have been diſcovered. One of thoſe ſeems to be the ſame with the red bark found in the cargo of the Spaniſh ſhip from Lima; the other ſeems to be one of the white ſpecies mentioned by Dr Simmons from the papers of the late M. Juſſieu.

bark of Dr Saunders, is a different ſpecies, or only a variety of the Cinchona officinalis of Linnæus, has not hitherto been fully determined. Having no information on this ſubject, but what I derived from Dr Saunders' book, I cannot pretend to give any opinion on a matter left altogether undetermined by thoſe who have had the beſt opportunities and talents to examine it. I ſhall therefore for the preſent content myſelf with the words of Dr Hope, in a letter to Dr Saunders: "If this red bark, which ſeems to be more efficacious than the common bark, can be had, that is the great point; and whether it be or be not the Cinchona of Linnæus, is but a ſecondary and much leſs important conſideration."

Dr Saunders is of opinion, that this red bark was that uſed in the days of Sydenham and Morton. With all due deference to the authority of Dr Saunders,

ders, I muſt beg leave to diſſent from him in this opinion. We find no very accurate deſcription of the bark in the works of Dr Sydenham; but, in thoſe of Morton, nothing appears to me more clearly, than that he uſed and preferred the thin quilled bark *.

The ſame author alſo tells us, that good bark is of a cinnamon-colour, which anſwers pretty nearly to that of the good pale bark now in uſe.

Neither do I think that theſe authors aſcribed to this remedy more remarkable ſucceſs than we ourſelves daily experience from the employment of the quill bark in common uſe. I would further obſerve, that Dr Sydenham, in his *Schedula monitoria de novæ Febris Ingreſſu*, inſtead of aſcribing any infallibility to the bark, is ſurpriſed at its want of ſucceſs *.

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* After marking the ſeveral teſts of good and adulterated bark, he concludes thus: "Corticem etiam, male electum, ſenſu facile deprehendimus, num ſcil. ſit craſſus et ligneus; quippe tenuis ille, qui a *ramulis* raditur, eſt maxime vegetus, quia ſucco recente ſaturatus."

I muſt alſo believe, that Sydenham meant nothing but the fallibility of the bark, when he adviſes the relapſes of tertians to be treated with his *decoctum amarum*.

We may likewiſe add, that Morton, when complaining of the adulteration of the bark, certainly experienced its fallibility †.

Authors have been at great pains to deſcribe the criteria of good and adulterated bark; but, till lately, theſe directions have been principally confined to the choice of the thin quilled bark.

The Abbé Raynal and M. la Condamine have told us, that the preference given

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[*] Mihi enim quantum potui diligenter perquirenti compertum fuit, etiam magnam corticis quantitatem exhibitam rariſſime ægrum a morbo liberaſſe.

[†] Quin et res ipſa idem clamitat: quare enim doſin ſolennem drcahmarum duarum adultis olim præſcriptam, hodie in febribus curandis ad tot uncias augere coacti ſumus.

given to the thin quilled bark was owing to ſome experiments in England. It is therefore probable, that the Engliſh, at that time, were either unacquainted with the red bark, or that theſe experiments were made on a red quilled bark, ſimilar to that lately ſeen by Dr Saunders, in a cargo imported by a Spaniſh merchant.

The qualities of good bark are moſt diſtinctly marked in the red bark of Dr Saunders; and we even preſume, that the goodneſs of every other kind may be eſtimated, as they approach more or leſs to the appearance of the red bark.

The red bark, therefore, is a heavy firm ſubſtance, is ſound and dry, has a bitter and aſtringent taſte, with a mixed kind of aromatic and muſty flavour. It is brittle and friable among the teeth, and leaves a laſting impreſſion on the tongue. It is of a red colour, and conſiſts of three diſtinct layers; "the external thin, rugged, and frequently covered with a moſſy ſubſtance, and of a reddiſh brown colour; the middle thicker, more compact, and of a darker colour; in this appears chiefly to reſide the reſinous part, being extremely brittle, and evidently containing a larger quantity of inflammable matter than any other kind of bark; the innermoſt has a more woody and fibrous appearance, and is of a brighter red than the former."

I muſt again, though unwillingly, diſſent from the opinion of Dr Saunders, who alledges, that, in reducing this bark to a powder, the middle will not give way ſo eaſily to the peſtle as the other layers. Dr Saunders himſelf has ſaid, that this layer is *extremely brittle*; and I preſume it would be reckoned a piece of bad pharmaceutical reaſoning, to maintain, that a body *extremely brittle* reſiſted the peſtle more obſtinately than a thin tenacious one. I have however attempted to determine this by experiments to be immediately related.

It may be proper to remark, that, in theſe experiments, I have often employed the red bark, when I had no direct view of aſcertaining the relative powers of each.

To avoid unneceſſary repetitions, I have not regarded the order in which they were made; but have attempted to connect ſuch experiments together, as I thought ſerved to illuſtrate each other.

EXPERIMENTS

ON THE

QUILL AND RED BARKS.

EXPERIMENT I.

HALF a pound of red bark was beat in a mortar for about an hour; it was then paſſed through a fine hair ſieve. The reſiduum weighed about three ounces. To two drachms of this powder were added ſix ounces of ſoft water.

EXPERIMENT II.

The reſiduum was beat about an hour and a half, and was then paſſed through the ſame ſieve. This powder was evidently paler than the firſt. To two drachms of it alſo I added ſix ounces of ſoft water.

EXPERIMENT III.

Having allowed both infuſions to ſtand about twelve hours, I added to two ounces of each a tea ſpoonful of my ſolution of martial vitriol.

The infuſion of Experiment I. was evidently darker coloured than that of Experiment II. and its taſte was conſiderably ſtronger.

From theſe experiments I am led to think, that the middle layer crumbles down firſt, and that the outer layer is of a tough epidermidal nature, not eaſily conquered by the peſtle. I imagined that I could perceive ſmall tough ſloughs in the reſidua, which I ſuppoſed to be parts of the outer and inner layers. I therefore believe that the ſtrongeſt powder comes firſt through the ſieve, and that this ought to be attended to, both in the practice of phyſic, and in making experiments. It is proper to obſerve, that my ſolution of *ſal martis* was in the proportion of ten grains of the ſalt to four ounces by meaſure of ſoft water. I invariably uſed theſe proportions through the courſe of my experiments. Theſe ſolutions ſoon depoſit an ochry matter; on which account, I prepared freſh ones as occaſion required.

EXPERIMENT IV.

Having macerated ſix ounces of the powder of quill bark in a gallon of ſoft water, I put the whole into a ſmall alembic, and drew off by a briſk heat a quart of diſtilled water. The water had ſenſibly the ſmell, but a very faint taſte of the bark. On its ſurface ſwam a thin coat, of a ſubſtance ſomewhat reſembling gum copal. On examining it with a microſcope, I was able to diſcern very minute globules of a ſpangling hue, inviſcated in the pellicular ſubſtance. Applying my tongue to it, I felt it evidently more pungent than the water, but much leſs ſo than I expected. I cannot however ſubſcribe to Neuman, who tells us, that the bark contains no eſſential oil. The ſpangling particles that appeared in the above mentioned experiment, were undoubtedly eſſential oil. I am at the ſame time diſpoſed to think, that it either exiſts in very ſmall quantity, or is ſo intimately combined with the other principles, that it is difficultly detached. My reaſons for applying a briſk heat will appear obvious, as I inſtituted the experiment chiefly with a view of obtaining the eſſential oil. The diſtilled water ſtruck no colour with a ſolution of martial vitriol. On ſtanding a very few days, it gradually let fall a kind of fleecy mucous ſediment, which kept regular pace with the loſs of its aromatic ſmell and taſte.

From this experiment I think we are warranted to conclude, that the bark loſes much of its aroma by boiling. It has been denied by ſome, that the bark was at all poſſeſſed of an aromatic principle. This however is very certainly a miſtake. I ſhould not even venture to aſſert, that the bark contained no more aroma than we are able to ſeparate by diſtillation. We know that the principles of bodies exiſt in different forms, and in various degrees of fixity. Few chemiſts, before the days of Stahl, would have believed that a piece of iron contained the ſame principle as alcohol, and perhaps in greater quantity. I wiſh not to be miſunderſtood. Every one knows, that iron and alcohol have very different effects on the living animal body. In this reſpect the analogy is far too diſtant for application. Chemiſtry, however, exhibits a multitude of cloſer propinquities. Our knowledge of primary principles, though limited and uncertain, is yet ſufficient to afford us conſiderable information on the nature of thoſe we call ſecondary. Of this laſt is confeſſedly the reſin of vegetables, which is ſuppoſed to be a combination of eſſential oil and acid. Nor do we know what ſeparation or other changes may go on in the proceſs of digeſtion.

I am the more willing to aſcribe to the bark ſome very fixed or other particular ſtate of its aroma, as I have been long led to ſuſpect, that its operation is more a ſtimulant power, than we ſhould expect from the quantity of that principle diſcoverable by experiment, or capable of being aſcertained by the ſenſes.

From theſe experiments I would further infer, that the principle of aſtringency is in a pretty fixed ſtate in the bark, as the diſtilled water imparted not the ſlighteſt tinge to a chalybeate. The reſiduum or decoction ſtruck a faint green colour with a ſolution of ſal martis. From the time the maceration was begun, till I made this experiment, the decoction was ſeven days old. I ſhall take notice of the green colour hereafter.

I preferred making this experiment with the quill bark, being that uſed by Neuman; and I was pretty certain, that whatever took place in the pale bark, as to ſtrength and ſenſible qualities, would alſo take place in the red, only in a much more eminent degree. I did not however repeat this experiment with the red bark, which indeed I very much regret *.

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* I have ſince had an opportunity of diſtilling the red bark in the New Diſpenſary of Edinburgh, in company with Dr Webſter. The reſult was much more evident than in my trials with the pale bark. The diſtilled water was of an albeſcent colour; and, on examining it with the microſcope, we were much amuſed with ſeeing the oil globules aſſume the appearance of large globes of ſilver dancing in a fluid.

As the reſiduum, or what chemiſts have called the *caput mortuum*, nowiſe differed from a decoction prepared for uſe, I was willing to make what experiments on that preparation were ſuggeſted to me at the time I was furniſhed with ſuch plenty of it. Having therefore ſeparated from it the reſinous depoſit, by ſtraining it whilſt boiling, and allowing it to precipitate when cool, I made the following trials.

EXPERIMENT V.

To two ounces of this decoction I added a tea ſpoonful of oleum tartari *per deliquium*. A lacteſcence immediately took place, and large flocci precipitated to the bottom of the liquor; the taſte was a neutral of bitter and alkaline; the clear liquor was little changed in colour.

EXPERIMENT VI.

To two ounces of the ſame decoction was added a ſmall quantity of mild volatile alkali. The ſame phænomena took place as in Experiment V.

EXPERIMENT VII.

To two ounces of the ſame decoction was added half a drachm of lixivium cauſticum. A maſs of ragged *flocculi* firſt waved through the liquor, part of them floated to the top, and after ſome time they all ſubſided to the bottom, though not ſo perfectly as in Experiments V. and VI. The colour was of a ruſty hue. From the day the diſtillation was performed, the decoction was now ſeven days old.

EXPERIMENT VIII.

To four ounces of the ſame decoction

was added an equal quantity of lime-water. A very large maſs of flocculi waved in the liquor, and ſoon after precipitated, leaving the clear liquor of a more intenſe red colour.

I found that this liquor had no taſte of lime-water. I was therefore induced to repeat the ſame experiment on the whole quantity I had of the decoction; and, meeting with the ſame reſult, I poured fixed air into the clear liquor, contained in a ſmall cylinder inverted in a baſon containing part of the ſame. I added the fixed air very cautiouſly, but was certain that no precipitation took place, either of calcareous earth, or colouring matter; the clear liquor was thin, but had a perceptible taſte of bitterneſs and aſtringency, though both were very faint.

EXPERIMENT IX.

On examining the matter depoſited

from the decoction by means of the lime-water, it had a ſlimy bitter taſte, leaving a ſenſe of roughneſs on the tongue.

EXPERIMENT X.

Imagining from the roughneſs this depoſit left on the tongue, that it had carried down with it much of the reſin of the bark, I added to a ſmall quantity of it a ſolution of martial vitriol. A dirty brown ſediment immediately took place, which deceived me at firſt, but I afterwards recollected that it was probably owing to the calcareous earth.

EXPERIMENT XI.

To diſcover more clearly the nature of this depoſit, I evaporated it to the conſiſtence of a rob; but, on carrying the evaporation further, it began to have an empyreumatic ſmell. On taking it off the fire, it had now acquired a ſenſible taſte of lime-water; which, I imagine, proceeded from the ſlight calcination of the calcareous earth on the ſides of the veſſel. The veſſel was one of thoſe called a *petty pan*.

EXPERIMENT XII.

I had now only about two ounces of this depoſit, and that too very thin: I however added to it ſix ounces of diſtilled water; and, putting the mixture into a ſmall but wide-mouthed phial inverted in a ſmall baſon containing part of the ſame, I impregnated it with fixed air, in the method propoſed by Dr Prieſtley. Having therefore forced out the liquor and agitated it into the phial four ſeveral times, I diſcontinued the proceſs, and allowed the liquor to ſtand for an hour. The depoſit was little diminiſhed in quantity. On pouring off the clear liquor, and adding to it ſome diluted vitriolic acid, it now made a ſlight effervescence. The depoſit had now no aſtringent taſte that I could perceive, but was ſimply bitter and ſlimy. It did not effervesce with acids, as before, nor could I perceive any precipitation or change of colour, on adding to it a ſolution of martial vitriol.

From theſe experiments, I think, we may conclude, that the addition of alkalis, fixed or volatile, is improper in preparing a decoction of the bark. This practice, I imagine, has ariſen from many different opinions. Some by this method ſuppoſed that they corrected an acid which, they alledged, was the cauſe of intermittent and other fevers. Others have conceived, that, by this addition, they reduced the reſinous matter to a ſaponaceous nature, more ſoluble in watery menſtrua.

Salt of wormwood is the particular alkali preſcribed by Mead and others, who, I ſuppoſe, imagined that by this means they added ſome additional bitterneſs to their preparations. Others have employed theſe ſalts as *deobſtruents*, as they called them, to obviate the ſuppoſed obſtructing power of the bark.

The notion of reducing the reſinous part to a ſaponaceous nature is plauſible in theory; but I ſhall afterwards attempt to ſhow that it cannot be reduced to practice.

The other opinions are perhaps too abſurd to require a ſerious refutation.

The ſenſe of roughneſs on the tongue, which the depoſit at firſt imparted, was probably owing to the calcareous earth, as, when this laſt was rediſſolved by the fixed air, no ſuch roughneſs was perceived.

I conducted Experiment XII. as quickly as I could, being apprehenſive that much of my depoſit would be rediſſolved in the water. This, however, was not the caſe; and I hence ſuſpect, that ſome real change is produced in the matter depoſited, and that the precipitation is not the mere effect of a ſeparation in conſequence of the ſuperior attraction theſe bodies have for water.

My decoction and depoſit being now conſumed, I made trials next on the reſinous matter depoſited from the plain decoction; but as theſe and ſundry other experiments of a like nature may be introduced more properly afterwards, I ſhall here relate my further trials with the lime-water.

EXPERIMENT XIII.

To one ounce of powdered quill bark I added one pound, by meaſure, of ſoft water. After allowing them to ſtand twelve hours, I poured off the clear liquor. To four ounces of this infuſion I added an equal quantity of lime-water. A turbeſcence immediately took place, and a flocculent cloud ſoon ſeparated to the bottom of the veſſel.

EXPERIMENT XIV.

After the precipitation of this flocculent cloud, I poured off the clear liquor, and added to a glaſsful of it a ſmall quantity of water impregnated with fixed air. A precipitation of calcareous earth immediately took place. I alſo poured into it fixed air from the pipe of my apparatus; and the ſame appearance was exhibited. It alſo rendered a ſolution of ſal martis dirty-coloured.

EXPERIMENT XV.

I boiled one ounce of powdered pale bark in two pounds, by meaſure, of water, till one half remained of the liquor. I ſtrained it whilſt boiling, and reſerved the reſin for other purpoſes. To four ounces of the clear decoction was added an equal quantity of lime-water. A turbeſcence and very copious precipitation took place immediately. I then poured off the clear liquor, and added to a glaſsful of it a ſmall quantity of the mephitic water. A lacteſcence immediately took place, and calcareous earth was precipitated; but not near ſo much as in the infuſion, though the quantities and proportions were alike in both. I imagined at firſt that I was deceived, when I found that it produced nearly the ſame colour with a chalybeate, as the infuſion. I therefore tried my other teſt, *viz.* of pouring the air into it from the pipe; but I ſtill found, that not near ſo much earth was precipitated as took place in the infuſion. It was therefore obvious, that though the calcareous earth was in leſs quantity in the clear liquor of the decoction, it was yet in ſufficient quantity to ſaturate the acid of the martial vitriol.

EXPERIMENT XVI.

To the matter ſeparated from the infuſion by means of the lime-water I added a ſmall quantity of diluted vitriolic acid. An exceedingly ſlight effervеſcence took place. The matter, depoſited in the ſame manner from the decoction, made, on the ſame trial, a much more ſenſible effervеſcence.

EXPERIMENT XVII.

To diſcover more accurately the action of lime-water on the bark, I reverſed the method of preparing Dr M'Bride's infuſion, as follows. To half an ounce of powdered red bark was added half a pound, by meaſure, of lime-water. Having allowed them to ſtand twelve hours, I poured off the liquor, which had a ſtrong taſte of lime-water, and was diſagreeably bitter. As Dr M'Bride has aſſerted, that, when fixed air is thrown into ſuch infuſions, the calcareous earth only is precipitated, I was therefore willing to give as ſtrong an impregnation as poſſible. For this purpoſe, I precipitated the lime from the infuſion by means of fixed air; but found that the calcareous earth did take down a *little* gummy depoſit, for the reaſon, I ſuppoſe, that it had but *little* to take down. Having decanted the infuſion off the calcareous earth, I again added it to the reſiduum; I added next to both two drachms of quicklime. When the whole had ſtood about an hour, I perceived a thin film of a gummy appearance interpoſed betwixt the lime and the body of the liquor. On ſhaking the veſſel, this film appeared again about half an hour afterwards, and continued to increaſe, I ſuppoſe, for the twelve hours that it ſtood. I again decanted off the clear infuſion, and, as formerly, poured no more fixed air into it than what was ſufficient to precipitate the lime. I again added the infuſion to the reſiduum, and, as formerly, added to both two drachms of quick-lime. I ſoon perceived the ſame kind of gummy film as above deſcribed. It increaſed for the firſt five or ſix hours; but, to my great ſurpriſe, it gradually afterwards became leſs and leſs, and at length diſappeared almoſt entirely. I now poured off the clear infuſion. It was very thick, and taſted ſtrongly of lime-water. I precipitated the lime from it by fixed air; but ſoon found, that a conſiderable quantity of gummy matter went down along with the earth, leaving the infuſion about as thin and weak as at firſt.

EXPERIMENT XIX.

To nearly two drachms of this depoſit was added three drachms of lixivium cauſticum. About two hours after, the depoſit was completely diſſolved. On this ſolution being ſaturated with fixed air, much of the depoſit ſubſided.

EXPERIMENT XX.

To a ſmall quantity of the reſinous matter depoſited from the decoctions were added three ounces of lime-water. No appearance of ſolution took place. The lime-water even retained in a great meaſure its own colour.

EXPERIMENT XXI.

To a ſmall quantity of the reſinous depoſit were added three ounces of lixivium cauſticum. A deep crimſon colour was ſoon produced, and the ſolution was complete in a few hours.

EXPERIMENT XXII.

To two ounces of a ſtrong infuſion of red bark was added half a drachm of lixivium cauſticum. A precipitation immediately began to form. On adding one half drachm more, the liquor became of a homogeneous conſiſtence, and remained ſo. It was pretty thick, and of a deep crimſon colour.

I ſhall now attempt to account for the reſult of the preceding experiments, as well as I can; and in this I am much aſſiſted by thoſe to be hereafter mentioned.

In Experiment VIII. we have ſeen, that an equal quantity of lime-water was decompoſed by a decoction of bark now ſeven days old. This decompoſition of the lime-water ſeems to be in ſome way connected with the ſtate and age of different preparations of the bark. In Experiment XIII. and XIV. this did not take place to ſuch a remarkable degree. In the freſh infuſion very little lime ſeems to have been precipitated; in the freſh decoction, however, a conſiderable quantity. I would hence conjecture, that boiling effects that change in bark, which is always the more perfect conſequence of age. From theſe experiments we muſt deduce one of two concluſions, *viz.* either that the infuſion contains leſs fixed air than the decoction, or that it contains it in a more fixed ſtate. That the infuſion contains more than the decoction, but in a more fixed ſtate, will, I think, be proved by experiments to be hereafter mentioned. Alſo that the decoction contains conſiderably leſs than the infuſion, but in a very looſe and evolved ſtate, will, in my opinion, appear equally evident.

I think it may appear obvious from theſe experiments, that lime-water is a very improper menſtruum for the bark.

In Experiment XVI. I have obſerved, that the matter precipitated from the infuſion efferveſced ſlightly with acids. Dr Percival did not carry his reſearches into this ſubject ſo far as might have been expected. But even in his trial, had he allowed his infuſion of bark in lime-water to ſtand not many hours, that ingenious and uſeful experimenter would have found, that the infuſion would have loſt its taſte of lime-water, and precipitated calcareous earth to the bottom of the veſſel.

From Experiment XVII. and XVIII. it appears, that the lime-water, in a ſmall quantity, is able to precipitate the gummy parts of the bark; yet, when applied in ſuch a manner, that a repetition of its force ſhall compenſate for its weakneſs, it is even capable of diſſolving the ſame matter it before depoſited. This is a curious fact; and, I imagine, is ſomewhat analogous to the action of fixed air on calcareous earth.

It is univerſally known, that lime-water is capable of diſſolving animal jellies; but few perhaps have ſuppoſed that it is by any means a ſolvent of vegetable mucilages, which are however very ſimilar in their ſenſible qualities to the gelatinous part of animals. Lime-water and cauſtic alkali are perhaps very ſimilar in their effects as ſolvents of bodies. I would even conjecture, that if we could accumulate a ſum of the powers of quicklime, in the form of lime-water, equal to that of cauſtic alkali, their ſolvent powers would be found exactly alike.

It is indeed to be obſerved, that, however ſimilar vegetable gum and animal gluten may be in their ſenſible qualities, their chemical analyſis afford very different products. It is equally obvious from the experiments of *Beccari*, *Parmentier*, and many others, that the concreſcible part of the *Farinacea*, (and perhaps of many other vegetables), conſiſts of two materials, which, though ſimilar in their ſenſible qualities, are yet very different in their chemical properties. It has been denied that this *glu-*

ten vegeto-animale was capable of ſolution in alkalis; but Mr M'Quer very certainly proved the contrary. As it is proved by fair experiment, that one of theſe materials is of an animal nature, and is that of the two which is moſt diſpoſed to fermentation, I would here conjecture, that the *gluten vegeto-animale* is more or leſs ſoluble in alkalis, as it contains more of the *gluten animale*, and leſs of the *amylum;* alſo that the fermentability of a vegetable in a greater or leſs degree, is an index of its containing more of the *gluten animale*, and leſs of the amylaceous part. Nor do I think that theſe ſpeculations will appear extravagant, when we conſider that the bark (according to Geoffroy) exhibited, on a chemical analyſis, very many of the properties of animal matter.

It would be foreign to my ſubject to enter into the celebrated doctrine of Hales, Haller, and eſpecially Dr M'Bride, on the ſubject of the cementing medium or *vinculum* of bodies. It is no doubt a fine idea, and was rendered plauſible by many phænomena in chemical philoſophy. I cannot however perceive how it can in any manner be uſefully applied in practice, at leaſt in the caſe now before us. Allowing that lime-water was capable of diſengaging this *vinculum*, how are we certain that this effect ſhould render theſe bodies more ſoluble in water? We have not only ſeen that lime-water precipitates a matter in decoctions of the bark, but alſo that this matter, when freed of its admixture with earth, ſeems to be leſs ſoluble in water than before. If lime-water had ſuch an effect, it is reaſonable to expect that it ſhould ſhow it leſs or more at the firſt inſtant. Beſides, we daily obſerve, that, ſo far from being probable that this ſuppoſed primary *vinculum* reſiſts the ſolubility of thoſe bodies in water, it evidently appears, that the ſpontaneous precipitation from infuſions and decoctions commences and keeps regular pace with the eſcape of this ſuppoſed *vinculum.*

I think I have diſcovered, that lime-water, with particular management, may be made to diſſolve the gummy parts of bark. But how are we to conclude that this is effected by the abſtraction of the fixed air, ſince cauſtic alkali, which does the ſame, is not thereby rendered mild, or capable of raiſing any effervefcence with acids? I muſt alſo obſerve, by the way, that ſuch ſolutions in lime-water are not only highly nauſeous, but often improper in practice.

On ſurveying the reaſoning of Dr M'Bride, we are indeed ſurpriſed that this ingenious and uſeful philoſopher, who truſted ſo much to the antiſeptic powers of the bark, ſhould adpot a method of depriving it of that very principle wherein he ſuppoſed its antiſeptic properties chiefly reſided. Nor can we think it a *deſideratum* in practice, that the bark ſhould be as much changed in its nature as happened to the mutton macerated in lime-water by Dr M'Bride. With bark reduced to ſuch a ſtate, the phyſician could no more cure fevers, than the cook with ſuch mutton could make an agreeable chop.

It may be worth obſerving, that lime-water, at leaſt in the way we employed it, had no obvious effect on the reſinous depoſit. After it had precipitated the gummy matter from the old decoction, the clear liquor imparted a ſenſible roughneſs on the tongue. From theſe facts I would infer, that the decoction (contrary to the aſſertion of Dr Lewis) does really contain reſin. Moſt part of the gummy matter having been now depoſited from the decoction, I would alſo infer, that water is capable of ſuſpending the reſinous matter, leſs dependent on the intervention of the gum than has been ſuppoſed; that the aſtringency of the clear liquor was more ſenſibly felt, becauſe the reſinous matter was now in a naked ſtate, or deprived of its ſheathing gum; and that lime-water diſſolved this gummy depoſit, by effecting ſome other change than the eſcape or abſorption of its fixed air, otherwiſe the water, according to Dr M'Bride, ſhould have been able to ſuſpend the gummy matter after the lime was precipitated. That part of the reſinous matter ſtill remains in the clear liquor, may, I think, be confirmed, from its taſte, and from the crimſon colour which it ſeems to acquire from the depoſition of the gummy part. This circumſtance of its colour may be illuſtrated by what every perſon would ſuppoſe *a priori*, *viz.* that an infuſion of cochineal and mucilage would be rendered much brighter by a ſeparation of the gum.

As I obſerved a freſh precipitation take place in the remainder of the clear liquor of the infuſion, and the liquor afterwards loſing the taſte of lime-water, I from this and other trials diſcovered that theſe experiments with lime-water might be uſed as teſts to compare the relative periſhable nature of different preparations of bark, and of different barks. I therefore proceeded as follows.

EXPERIMENT XXIII.

Though the following experiments were not all made on the ſame infuſions and decoctions, yet they were made on infuſions and decoctions of the ſame age and ſtrength, as marked in my notes. I ſhall, therefore, to avoid repetitions, connect them here in the beſt order I can.

An infuſion and decoction of pale bark were made of equal ſtrength, (I mean with equal proportions of bark and water). To four ounces of each was added an equal quantity of lime-water. After ſix days, the decoction ceaſed to have a taſte of lime-water. I added to it other two ounces of lime-water. A very ſmall precipitation took place; but it retained a ſlight taſte of lime-water, which continued ſo long as I kept it. The infuſion ceaſed to have a taſte of lime-water on the ſixth day alſo. To it likewiſe I added two ounces of lime-water. A conſiderable precipitation took place, and the infuſion had no taſte of lime-water. On adding another ounce of lime-water, it again acquired a ſlight taſte of the lime, which diſappeared in a few hours. I again added an ounce of lime-water; but it now retained its taſte of lime-water as long as I kept it.

From this experiment we ſee, that the bark very ſoon runs into fermentation, and that the decoction affords leſs fixed air than the infuſion. I had perhaps not examined the decoction ſoon enough, other buſineſs often coming in the way; but I was certain, from other trials, that the decoction ferments ſooneſt.

From the infuſion decompoſing a greater quantity of lime-water, it is evident that it contains more fixed air than the decoction. From this air being in a more fixed ſtate in the infuſion than in the decoction, the former muſt be leſs periſhable than the latter. And, laſtly, that cold water extracts the principles of the bark in a more undecompoſed ſtate than boiling.

EXPERIMENT XXIV.

An infuſion and decoction of red bark were made in the ſame proportions as the preceding. To four ounces of each was added an equal quantity of lime-water. The decoction ceaſed to have a taſte of lime-water on the eighth day. The infuſion retained a conſiderable taſte of lime-water till the fourteenth day. I added more to the decoction, till it had a taſte of lime-water, in all four and a half ounces. The infuſion required nearly double that quantity to retain a permanent taſte of lime-water. The precipitations were more copious from the red than the pale bark.

Had I known what after-experiments taught me, I would not have added the lime-water when the infuſions and decoctions were freſh, as in this way they ſooner loſe their taſte of lime-water.

From theſe experiments I would infer, that decoctions and infuſions of red bark are leſs periſhable than thoſe of the pale; that they furniſh not only a greater quantity of fixed air, but alſo a more copious depoſition of the extractive matter of the bark.

As my experiments were made by adding the lime-water whilſt they were freſh, I cannot conclude, that thoſe preparations would keep unfermenting the length of time only that the decompoſition of the lime-water took place. I before obſerved, that, after theſe experiments were made, I found that lime-water conſiderably accelerated the ſeparation of their fixed air, and its own decompoſition*.

I muſt own, that I was very much ſurpriſed at the reſult of theſe experiments. I could not have conceived that bark of any kind could furniſh ſuch an immenſe quantity of fixed air. That part of this gas may be a child of age and fermentation, I ſhall not venture to deny; but from the freſh decoction furniſhing it in conſiderable quantity, I am led to think that a great part or moſt of it exiſts ready formed in the bark.

I ſhall now relate ſome experiments conducted on a different plan.

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* The lime-water imparts a nauſeous ſmell and taſte to a freſh infuſion; it however prevents the putrid ſmell which theſe preparations acquire by ſtanding.

EXPERIMENT XXV.

To two ounces of infuſion of pale bark, whilſt freſh, I added a tea ſpoonful of my chalybeate. I alſo mixed the ſame quantities and proportions of the decoction and chalybeate. The decoction ſtruck a pretty deep purple in a very ſhort time. The infuſion was longer in ſtriking any colour, but, on ſtanding, became much darker coloured than the decoction.

EXPERIMENT XXVI.

To two ounces of the infuſion of pale bark, now four days old, was added a tea ſpoonful of the chalybeate. I alſo mixed the ſame quantities and proportions of a decoction four days old and a chalybeate. The infuſion now ſtruck a purple colour much more ſuddenly than when freſh, but did not become darker by ſtanding. In the colour, and quick appearance of the colour, it much reſembled the decoction of Experiment XXV. The decoction inſtantly ſtruck with the chalybeate a greeniſh colour.

EXPERIMENT XXVII.

To an infuſion eight days old I added a chalybeate; a beautiful green colour was inſtantly produced. To a decoction of about the ſame age and ſtrength I alſo added a chalybeate. A green colour was immediately produced, but conſiderably lighter than in the infuſion.

This green colour I had very early diſcovered by accident, and was much puzzled to account for it; neither could I find that any author had taken notice of it. I have ſince, however, found, that Dr Lewis had alſo obſerved this appearance, and had therefore ſuppoſed that the bark was an exception from other aſtringents on this account. I am not a little ſurpriſed, that the accurate Dr Percival, who ſeems ſo much diſpoſed to examine the opinions of that learned and judicious chemiſt, ſhould not have obſerved this circumſtance of the green colour, either in Dr Lewis's works, or in his own experiments. For my own part, I remember perfectly well, that I firſt obſerved it in making experiments with an old infuſion. I muſt therefore ſuſpect, that Dr Lewis, however accurate in other matters, was in this deceived, by not attending ſufficiently to the age of his preparation.

Other experiments diſcovered to me its true nature, and furniſhed me with ſome uſeful teſts in examining the ſtate of different preparations of the bark; but as my obſervations on the nature of ink are, ſo far as I know, in a great meaſure new, I have allotted to that ſubject a diſtinct part of this eſſay, where I ſhall attempt to account for the preceding and ſome of the following experiments.

EXPERIMENT XXVIII.

I ſhall not here relate the minute circumſtances of the experiments comprehended under this diviſion, but obſerve once for all, that both infuſions and decoctions of the red bark ſtrike a darker colour with chalybeates than thoſe of the pale. I alſo obſerved the very ſame circumſtances (reſpecting the age and colour) in the decoction and infuſion of the red as in thoſe of the pale bark; with this difference, that both thoſe of the former were much older before they ſtruck a green colour with the chalybeate. The decoction did not produce a green colour till it was fourteen days old; the infuſion, not till it was about three weeks old.

From theſe experiments I infer, that the preparations of red are not only much ſtronger than thoſe of the pale bark, but are alſo much leſs periſhable. I think we may alſo ſee the fallacy of eſtimating the ſtrength of a vegetable aſtringent by the colour it ſtrikes with chalybeates. The ſudden appearance of the colour is, I apprehend, to be conſidered as a ſign of the weakneſs of ſuch preparations. Hence I imagine that Dr Percival might have conſidered the cold infuſion as the ſtrongeſt, becauſe it ſtruck the colour more ſlowly, but was darker than the decoction, after it had ſtood for ſome time. It may however be ſaid, that this effect may take place from the evolved ſtate of the fixed air in the decoction, when the principle of aſtringency may remain the ſame as in the infuſion. It is however obſervable, that the ſtrength of theſe preparations ſuffers by, and keeps regular pace with, the ſeparation and eſcape of their fixed air. I am hence led to think, that fixed air conſtitutes an eſſential part of this principle of aſtringency. Allow me the boldneſs to conjecture, that a combination of fixed air, as an acid, with the eſſential oil, may by ſome particular modification conſtitute the reſin itſelf. It is here proper to obſerve, that a weak infuſion of the bark, though freſh, ſtrikes no green colour with chalybeates. I therefore conclude, that the more ſudden production of colour in the decoction was owing * to a more evolved ſtate of this gas than in the infuſion; and that the green colour not only proceeded from a ſtill more evolved ſtate of this

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* Since this paper was given in to the Harveian Society, I have rather altered my opinion on this ſubject. I think we have no evidence from theſe experiments, nor indeed from any analogy of the effects of fixed air on iron or other bodies, to make us ſuppoſe the various ſtates of this gas as the *cauſe* of theſe different circumſtances in the production of ink. I will therefore, for the preſent, only conſider theſe different appearances in the colour ſtruck by chalybeates, as criteria or *ſigns* only of the different conditions of infuſions and decoctions.

this gas, but alſo from there being a leſſer quantity in the preparation.

On theſe principles, I would alledge, that any proceſs tending to diſengage the fixed air of the bark muſt be hurtful in preparing it.

I ſhall diſmiſs this part of my ſubject, by making ſome application of the above experiments, as teſts of the ſtate of different preparations of the bark.

The more ſuddenly any preparation of the bark ſtrikes a colour with chalybeates, we may be the more aſſured that the preparation is in a fermenting ſtate. If the colour is purple, the fermentation has not yet far advanced; if it is of a dark green, the fermentation has advanced a little farther; if, again, the green is of a lighter hue, it is ſtill farther advanced; but if no colour is produced, which, by the by, I never knew to take place, I ſhould then conclude, that a total avolation of this gas has happened.

Having thus found that chalybeates were ſuch a fallacious teſt of the *original* ſtrength of different preparations of the bark, I was led to pay more particular attention to the age and ſtate of theſe preparations, before I formed any decided opinion. I indeed ſuppoſe, that every perſon making ſuch experiments will find, that the ſenſation on the tongue is the moſt unexceptionable criterion of the aſtringency of any vegetable whatever. I however imagine, that, with the above precautions, chalybeates are not only uſeful teſts of their age and ſtate, but are conſequently excellent criteria of their preſent freſhneſs and ſtrength.

As Dr Percival has made many experiments, than which none can be more deciſive and well directed, I thought I could not ſatisfy myſelf better on this ſubject than by following the ſame ſteps.

EXPERIMENT XXIX.

I therefore tried Dr Percival's ſecond experiment of boiling the *reſidua* of the decoction and infuſion. I concluded from my trials alſo, that the decoctions were of equal ſtrength. I muſt however ſuſpect, that this by no means proves that the original decoction was equally ſtrong as the infuſion. From other experiments I would infer, that in both *reſidua* as much reſin remained as boiling water is able to ſuſpend. To illuſtrate this, in

EXPERIMENT XXX.

I tried and found, that an infuſion of the reſiduum of the decoction was ſtronger than a decoction of the ſame *reſiduum* in the ſame proportions of reſiduum and water.

EXPERIMENT XXXI.

I alſo found, that an infuſion of the reſiduum of the firſt infuſion was ſtronger than a decoction of the ſame reſiduum in the ſame proportions of reſiduum and water.

I could not account for the reſult of this laſt experiment at the time I made it. I however think, that I afterwards diſcovered, that boiling is able to deſtroy the aſtringency of the bark entirely, and always impairs it leſs or more. I am ſorry I did not carry theſe experiments farther; but I did not then ſo fully ſee their importance. I would now conclude, that the cold infuſion of the bark is in many reſpects preferable to the decoction.

Dr Lewis has told us, that the reſin of the bark is in no degree ſoluble in water. Others have compared infuſions and decoctions to an emulſion. That the gum aſſiſts in the ſuſpenſion of the reſin, I will not deny; but that the reſinous part is inſoluble in water, is, I think, evidently a miſtake. I apprehend that few experiments might have been neceſſary to ſatisfy us of this; I believe it is an eſtabliſhed principle in phyſiology, that we feel not the taſte of bodies, but in a ſtate of ſolution. Now, we certainly do feel an aſtringent taſte in the reſin of the bark; which, I imagine, would never happen, unleſs that reſin was ſoluble in the ſaliva.

Before I go farther, it is here proper to obſerve, that, in mentioning the preparations with which I conducted theſe experiments, when I ſpeak of a decoction being of equal ſtrength as an infuſion, I mean the proportion of an equal quantity of bark boiled in a double quantity of water, till one half of the liquor (or the quantity of the infuſion) is evaporated. In like manner, when I ſpeak of decoctions or infuſions of red and pale bark being of equal ſtrength, I mean relatively to the proportion of an equal quantity of bark and water.

EXPERIMENT XXXII.

I ſeparated the reſinous depoſits of the decoctions of the reſidua of the original infuſion and decoction; and found that, as near as I could gueſs, an equal quantity was depoſited from both. This reſult directed me much to the inference I have drawn in explaining the nature of Experiment XXIX.

From all theſe experiments, (which, it is proper to obſerve, were made on the pale bark), I think we may infer, that the practice of throwing away the reſidua of infuſions and decoctions of bark is really a piece of profuſion. They may be applied to very uſeful purpoſes, more eſpecially, as we ſhall ſhow, in the making of tinctures. I would even ſuſpect, that the reſiduum of an infuſion of red bark would be little leſs ſtrong than genuine powder of pale bark. It however never occurred to me to make this experiment.

I think it unneceſſary to relate the particular experiments on the triturated infuſion. I would, however, obſerve, that ſuch ſuſpenſions are by no means complete. I always found that thoſe infuſions let fall a quantity of ſediment, which was no more than fine alcohol of bark; but, even after this depoſition, I ſtill thought the infuſion ſtronger than thoſe made without trituration. When I ſpeak of ſtrength, I mean of the bitter and aſtringent parts; for I am pretty certain that the aromatic ſuffer by this proceſs, as, during the trituration, a ſtrong ſmell of bark is diffuſed and diſſipated in the circumambient air. It did not appear to me that a decoction of the reſiduum of the triturated infuſion was ſenſibly weaker than a decoction of the reſiduum of the plain infuſion. But Dr Percival thinks it was.

EXPERIMENT XXXIII.

I boiled equal quantities of the ſimple and triturated infuſions. The triturated infuſion became conſiderably more turbid than the other; and, on cooling, let fall a greater quantity of ſediment. I again boiled both, and ſtrained them through a double cloth; but found that a ſediment ſtill ſubſided from the triturated infuſion. I again ſubjected them both to the boiling heat, and ſtrained them, whilſt boiling, through a very thick layer of lint. The clear decoctions, on cooling, precipitated, ſo far as I could judge, an equal quantity of reſinous depoſit.

From this experiment I am led to think, that trituration aſſiſts the ſuſpenſion of the bark in water much in the form of emulſions. The method, therefore, propoſed by Dr Fothergill, may, under certain circumſtances, be uſeful. I preſume, however, that it can only be applied for the ſuſpenſion or diffuſion of fine powder of entire bark; as, if we were to add theſe matters in making an infuſion, we ſhould certainly ſo burden the ſolvent powers of the water, that leſs of the ſoluble matter of the bark would be taken up. I think this experiment not only proves that reſin is diſſolved in an infuſion of the bark, but alſo that the ſame reſin is made incapable of further ſolution by means of boiling. The gummy part alſo is now depoſited in conſiderable quantity.

EXPERIMENT XXXIV.

I infuſed an ounce of bark in twelve ounces of ſoft water, and let them ſtand three days. The clear liquor was then decanted off, and the ſame quantity of freſh water added to the reſiduum. For reaſons I ſhall afterwards mention, I diſcontinued the term of three days infuſion, and only allowed twelve hours for each. This proceſs was repeated twenty-two times. The water then came off clear and inſipid. The reſiduum was of conſiderably leſs bulk than the original powder, and exhibited a gritty and ſomewhat filamentous look and feel. I infuſed two drachms of this reſiduum in an ounce of rectified ſpirit. After ſtanding forty-eight hours, the infuſion acquired a pale colour. Its taſte was conſiderably bitter, but I could perceive no ſenſible aſtringency.

EXPERIMENT XXXV.

I boiled the remainder of the reſiduum in twelve ounces of water. A very ſlight yellowiſh colour was perceived. I inſtantly ſtrained it through lint; but before I could venture to taſte the decoction, it let fall a ſmall quantity of a blackiſh powder, after which the liquor was clear and taſteleſs. This black powder had much of the appearance of reſin, but did not impart any ſenſible roughneſs to the tongue.

Theſe experiments were made with the pale bark, being that which I had in greateſt plenty at the time.

It may appear extraordinary, that, in Experiment XXXIV. twenty-two affuſions of water ſhould have exhauſted the ſtrength of my bark, when it required thirty ſuch, and theſe continued three days each, to exhauſt that of Dr Percival. From comparing many of my experiments with Dr Percival's, I am led to think, that his bark muſt have been much ſtronger than my pale bark. I think I can alſo perceive a reaſon why

Dr Percival's bark required a greater number of affuſions than mine, and for which I left off allowing the infuſions to ſtand ſo long as his did. When my infuſion remained much longer than twelve hours, I obſerved a pretty thick layer interpoſed betwixt the powder and the body of the liquor. I imagined, on firſt obſerving it, that it might be owing to a ſolution of part of the gummy matter, not yet diffuſed through the infuſion: but, on ſhaking the glaſs briſkly, the ſame appearance very ſoon returned; which convinced me, that cold water, after having diſſolved a given quantity of bark, is at a ſtand, and precipitates that which at firſt it diſſolved in an imperfect ſtate. However that be, it very certainly becomes weaker by ſtanding after ſuch an appearance takes place. Hence the impropriety of allowing bark to ſtand too long in infuſion. On theſe principles I would partly account for the difference betwixt Dr

Percival's experiments and my own. I would even ſuſpect, that each new affuſion would gain nearly as much from this gummy layer as from the reſiduum itſelf. This experiment alſo renders it probable, that the reſin is ſoluble in water without the intervention of gum. We know that gums have almoſt no limitation in their ſolubility in water, and acquire a thickneſs equal to balſams; but we here ſee evident limits to the ſolubility of the matter of the bark. Is not this probably owing to the water being already ſaturated with the reſin, which ſeems to be as little liable to ſpontaneous ſeparation as the gum itſelf?

From Experiment XXXV. I imagine, that boiling water does not ſuſpend reſin as a ſolvent, but effects what may be called a temporary fuſion. I think it will appear reaſonable to ſuppoſe the blackiſh powder of a reſinous nature, as it had all the external appearance of that matter. Its want of taſte might be owing partly to the boiling, but principally to its inſolubility in the ſaliva. I think the experiment of the ſpirituous infuſion proves, that all the uſeful parts of bark are not to be extracted by water alone: It is probable the ſpirit might have extracted more, had the infuſion been allowed to ſtand longer.

EXPERIMENT XXXVI.

I next proceeded to try the effect of repeated coctions. I followed Dr Percival's general plan, but did not incloſe the powder in a bag. I ſtrained the boiling decoction each time through a ſmall double cloth. The reſiduum now remaining in the cloth, I boiled both together in the freſh parcels of water. It is proper to acknowledge, that I employed the reſiduum of a former decoction in this experiment. After thirteen coctions, the liquor came off clear and taſteleſs. I imagine it might have taken about fifteen coctions, if freſh bark had been uſed.

EXPERIMENT XXXVII.

To two drachms of the reſiduum I added one ounce of rectified ſpirit; but, to my no ſmall ſurpriſe, the tincture was more inſipid than the tincture of the reſiduum of the infuſion. It was indeed conſiderably weaker.

I think I can perceive how Dr Percival has been miſled in this experiment. He does not relate the particulars of his proceſs; but, if he conducted the coctions as he did the infuſions, to wit, by allowing them to ſtand for ſome time, and then decanting off the clear liquor, it is obvious, that along with his reſiduum he would have the reſinous depoſit of each decoction. In my trials, I obſerved a reſinous matter depoſited from the firſt even to the ninth decoction, and there was nearly as much in the fourth as in the firſt coction. After the fourth, however, it became gradually leſs, and aſſumed a blackiſh colour. I muſt therefore ſuſpect, that Dr Percival's tincture was made up of a quantity of this reſinous depoſit. From the firſt eight coctions I had collected a conſiderable quantity of this matter. It is indeed probable, that a great part of this depoſit would be diſſipated, or (if I may be allowed the expreſſion) decompoſed by the boiling employed by Dr Percival. It is however probable, that a very minute quantity remaining would be ſufficient to impregnate ſuch a ſmall quantity of rectified ſpirit.

From this experiment I would infer, that repeated coctions are capable of melting out moſt part or perhaps all of the reſin of the bark. One would ſuppoſe, that if the heat was the ſole agent in this proceſs, the reſin would all melt out in the firſt boiling; but this is evidently not the caſe, though Dr Lewis has aſſerted the contrary. From this experiment I was led to ſuſpect, that very long boiling might entirely decompoſe the principles, and more eſpecially the reſin of the bark. I therefore made the following trial.

EXPERIMENT XXXVIII.

I had by me a conſiderable quantity of the reſidua of infuſions and decoctions of red and pale bark, which, when mixed and dried a little, were tolerably ſtrong. I boiled three ounces of theſe reſidua in a gallon of water. When the liquor was evaporated to about three Engliſh pints, I added another gallon. I continued boiling and adding freſh parcels of water for four days, including much of the night alſo. When I took out ſome of the decoction from the beginning of the boiling, and ſtrained it through lint, it depoſited, when cool, a very conſiderable quantity of reſinous matter; but after this long continued boiling, when I ſtrained the whole boiling decoction through lint, it depoſited, when cool, about the ſame quantity of reſinous matter that a quart of the decoction at firſt did. This depoſit was quite black, and had no taſte that I could diſcern. The decoction was of a pale colour, and had a faintly bitter taſte, accompanied with a kind of diſagreeable harſhneſs.

EXPERIMENT XXXIX.

To four ounces of this decoction I added two ounces of lime-water. A ſlight turbeſcence took place, with ſome little precipitation. I kept the mixture about ten days; but it ſtill retained its taſte of lime-water.

EXPERIMENT XL.

To two drachms of the reſiduum of this decoction I added one ounce of rectified ſpirit of wine. The infuſion gained ſome little colour; but I could not perceive in it any ſenſible taſte.

From theſe experiments, I think, we may infer, that the watery extract of the bark does not contain thoſe principles that are ſoluble in water, I mean in their natural ſtate and integrity. A great part of the aſtringency muſt be diſſipated or decompoſed by the boiling. On the other hand, I conjecture, that, after a thouſand affuſions of cold water, we ſhould ſtill be able to obtain a conſiderably ſtrong tincture from the reſiduum, as the bark does not appear to ſuffer any change of its principles from the action of cold water.

It is here proper to obſerve, that, though the reſinous depoſit aſſumed a black colour, it by no means reſembled the nature of coal, as it retained its uſual feel and ſome degree of tenacity, which laſt all theſe depoſits poſſeſs from the admixture of a gummy matter.

I next tried Dr Percival's eighteenth experiment, and was not a little ſurpriſed at the reſult. The affuſion of the rectified ſpirit on the bark was repeated ſix times, at forty-eight hours diſtance. Though the reſiduum was altogether inſipid on the tongue, it yet communicated a conſiderable aſtringency to cold water. I was at firſt at ſome loſs how to reconcile this fact with my former idea, that a ſolution of bodies was neceſſary to taſte. It is, however, probable, that what is ſoluble in water is alſo ſoluble in ſaliva, if this laſt were long enough applied to the ſolvend. I think it may alſo prove, that thoſe bodies not readily ſoluble in the ſaliva muſt remain inſipid till we make an infuſion in the mouth, which would be very inconvenient, or perhaps impoſſible.

What was moſt ſurpriſing, however, in this experiment, was, that, according to Dr Percival's experiments and my own, the watery infuſion was not in the leaſt degree bitter. It therefore appears, that the rectified ſpirit is capable of extracting all the bitterneſs of the bark, but not all its aſtringency.

From theſe united experiments we ſee, that the properties of the bark are neither to be extracted by water, nor rectified ſpirit, ſeparately employed. A combination therefore of the two is undoubtedly the moſt complete menſtruum of the bark. To determine this, I made the following experiments.

As I imagined that Dr Percival's method of weighing the reſidua would be a very troubleſome and ambiguous proceſs, I reverſed his method, and weighed the liquors themſelves. As I had found by a previous experiment, that the proportion of a drachm and a half, both of the pale and red bark, to three ounces of water, made, as nearly as I could judge, the infuſions as ſtrong as they could be, I therefore made uſe of that quantity and proportions. But as I had made no ſuch trials with the other liquors, I was obliged to prepare them at random, in the ſame proportions as the watery infuſions.

The weight ſet down in the following column is that by which the different infuſions exceeded the liquors in which they were infuſed. The quantity weighed was two ounces of each. I have alſo ſet down the term of infuſion.

Powder of Pale Bark, ʒiſs in ℥iij.

Soft water,	12 hours,	3½ grains.
Rectified ſpirit,	7 days,	3
French brandy,	7 days,	4½
Rheniſh wine,	7 days,	4 and odds.
Sp. nitr. dulc.	7 days,	3½ and odds.
Vinegar and water, by Dr Percival's proportion,	12 hours,	4

Lixivium cauſticum,	3 days,	—	5 nearly.
Ol. tartar. pr. deliq.	3 days,	—	2 and odds.
Mild volatile alkali,	3 days,	—	$3\frac{1}{2}$
Lime-water, —	6 hours,	—	2 and odds.
Sp. vitriol. dulc.	7 days,	—	$6\frac{1}{2}$.

I cannot account for the differences in the reſult of theſe experiments from thoſe of Dr Percival, except, as I have all along ſuſpected, that his bark has been of exceeding good quality, far indeed ſurpaſſing mine. Every one alſo muſt be ſenſible, that, in weighing ſuch ſmall quantities, there is very much room for miſtake. I muſt likewiſe ſuſpect, that the method followed by Dr Percival, of eſtimating the weight by the loſs of the reſiduum, is ſtill more fallacious than weighing the liquors themſelves. Neither do I think it fair, that they ſhould all remain in infuſion for the ſame length of time. This did not eſcape the accuracy and obſervation of Dr Percival himſelf, who has preſcribed the term of ſeven or eight hours as the *ne plus ultra* of the infuſions in cold water.

ter. I cannot be certain whether I let the reſt ſtand the proper time or not. I was in this principally directed by examining from time to time for any appearance of depoſited matter betwixt the bark and the body of the liquor. But though this applies very well in judging of an infuſion in cold water, it is wanting in many other infuſions; and in thoſe where it did appear to take place, I could not always be certain of its nature.

My Rheniſh wine appeared to diſſolve leſs than proof ſpirit; but theſe experiments will, no doubt, vary from the different qualities of the liquors employed. This laſt objection cannot be urged againſt the following experiments, made with a view of comparing the relative ſtrength, or rather ſolubility, of the red and pale barks: I ſay, rather ſolubility; for I have lately found, that the proportion of a drachm of red bark to three ounces of water is nearly as ſtrong as that of a drachm and a half with the ſame quantity of water; which, I think, proves that the parts of this bark are not only more ſoluble in water, but are perhaps alſo of a more active nature.

The following experiments tend chiefly to aſcertain the relative ſolubility of the two barks. They were made exactly in the manner of the preceding, and are to be underſtood in the ſame way.

Powdered Red Bark, ʒiſs in ℥iij.

Cold water,	12 hours,	$4\frac{1}{7}$ and odds.
Rectified ſpirit,	7 days,	$3\frac{1}{2}$ and odds.
French brandy,	7 days,	6 and odds.
Rheniſh wine,	7 days,	$6\frac{1}{2}$.

I tried no other menſtrua, being perſuaded that red bark is more ſoluble than the pale in any menſtruum whatever. I was now ſurpriſed to find that the Rheniſh wine diſſolved an equal, or perhaps a greater quantity than the French brandy. I could not perceive that any miſtake had happened either in theſe or the preceding experiments. The Rheniſh wine was taken out of the ſame bottle for each, and both were infuſed and weighed on the ſame days.

I think theſe experiments clearly prove the ſuperiority of the red to the pale bark.

If from the above experiments we calculate the quantity of matter ſuſpended in two pounds (*i. e.* an Engliſh quart) of the cold infuſion, where the ſame proportions are uſed as in the above trials, we ſhall find, that, in a quart of the infuſion of pale bark, there is ſuſpended betwixt fifty and ſixty grains.

If, in the ſame way, we calculate the quantity ſuſpended in an infuſion of the red bark, we ſhall find it to be about a hundred grains. It is obvious, that the infuſions in French brandy and Rheniſh wine muſt contain a good deal more.

From theſe experiments it appears, that infuſions and decoctions of the bark contain but a very ſmall proportion of its parts. That the ſuſpended matter may be more active than entire bark, I will not deny; I alſo think, that the red bark is ſtronger than the pale, quantity for quantity; but to truſt the cure of intermittents to ſuch preparations, would, I preſume, be a ſource of miſerable diſappointments. In caſes, however, of great debility, their effects, as relative to the ſtate of the ſtomach at the time, may be more powerful than bark in ſubſtance. But in diſeaſes of any violence, and where entire bark can with propriety be employed, I apprehend that its decoctions and infuſions ſhould be uſed more in the form of ſmall beer and gruel, than as medicines we can in any meaſure depend on.

Many practitioners order the tincture of bark to be added to theſe preparations in the proportion of a twentieth part. I muſt ſuſpect that this is a piece of bad pharmacy. As the ardent ſpirit is no longer able to keep ſuſpended the matter of the bark when it is reduced with water, it is certainly more prudent to mix the ſpirit with the water at the commencement of the infuſion.

As orange-peel, and ſuch like ingredients, muſt burden the ſolvent powers of the menſtruum, I would propoſe, that, when we wiſh to make the addition of an aromatic, it ſhould be in the form of ſome ſpirituous water, as follows:

℞ Pulv. Cort. Peruv. ℥ſs. infunde in
Aq. Cinnamom. ſpirituoſ. ℥viij.
To ſtand twelve hours.

Where ſpirits are improper, or diſagreeable, the bark might be infuſed in the ſimple waters, as thoſe of cinnamon and peppermint.

As, from preceding experiments, I was pretty certain, that I could procure the reſin, as extracted by rectified ſpirit of wine, at a very inconſiderable expence, I infuſed half a pound of powdered red bark in two quarts of a whiſky which was ſtronger than any rectified ſpirit of wine that I could procure. I allowed the infuſion to ſtand ten days; after which it had acquired a very deep crimſon colour, but could not be filtered through paper on account of its thickneſs. After, therefore, decanting it carefully off, I put it into a ſmall alembic, and drew off, by means of a very gentle heat, near a quart of highly rectified ſpirit, having a ſmell ſomewhat like aniſeſeeds. The caput mortuum was very thick and heterogeneous. Having put it into a very large glaſs cylinder, I added to it plain water till the veſſel was full. A very copious precipitation took place in the liquor, but did not ſink freely to the bottom of the veſſel. I therefore filtered the whole through paper; which done, I collected from the papers about an ounce and a half of reſinous matter, of a conſiſtence ſomewhat paſty. I divided it into two equal halves, and committed one half to the care of a baker, who I deſired might not put it into his oven till the thermometer fell to 212. I ſuſpect it was put in when the oven was conſiderably warmer. However that be, the taſte of the reſin, which before was prodigiouſly aſtringent, was now almoſt deſtroyed. Its colour, which before exactly reſembled that of its bark, was now changed to a blackiſh hue, reſembling that which I had before ſeen in the depoſits of repeated coctions. It was not however a coal, as it ſtill retained ſome moiſture and tenacity; it was much diminiſhed in bulk, and ſtuck pretty firmly to the plate on which it was ſpread.

I put the reſin into the oven, with an intention of drying it, ſo as to render it more manageable for future experiments. The above accident, however, taught me, that the aſtringency of the bark is by no means ſuch a fixed principle as I had ſuppoſed.

I tried to free the other half of its

adhering gum; but I ſoon found, that its bulk was perceptibly diminiſhed on each affuſion of the water, and the clear liquor had a manifeſt aſtringency.

By the time theſe proceſſes were finiſhed, the filtered liquor had depoſited a conſiderable quantity of the ſame colour, and, as I judged, of the ſame quality as the firſt. I therefore drew off by diſtillation all the ſpirit that remained; at leaſt, till that which came from the worm was equal in weight to diſtilled water.

On examining the caput mortuum, it was found to have acquired a darker colour; which however it loſt on the depoſition of a reſinous matter, the colour of which reſembled that of opium. Its conſiſtence was ſomewhat more glutinous than the firſt reſin. I obtained this, as the former, by filtering the liquor, and collecting it off the papers. The quantity was about half that of the former, or ſcarce ſo much. It had an aſtringent taſte, but not nearly ſo conſiderable as the firſt reſin; it had alſo a harſher and more diſagreeable taſte.

I next proceeded to try the ſolubility of the reſin in different menſtrua. I had now only about three drachms of the firſt reſin remaining, and I could not weigh it without much loſs. I therefore put equal quantities of it, as nearly as I could gueſs, into the following liquors, whoſe ſolvent powers ſeemed to me to ſtand in the following order.

Dulcified ſpirit of ſâl ammoniac.
Cauſtic volatile alkali.
Cauſtic fixed alkali.
Dulcified ſpirit of vitriol.
Dulcified ſpirit of nitre.
Rectified ſpirit of wine.
Proof ſpirit.
Water.
Lime-water.

I added of each of theſe liquors three

drachms by meaſure to, I ſuppoſe, upwards of ten grains of the reſin. I remember that I was very attentive to theſe experiments, but did not weigh the liquors. I do not however conclude, that thoſe ſtanding higheſt in the liſt are the beſt menſtrua for entire bark. Some of them are inimical to the ſuſpenſion of the other principles of the bark; others are by much too acrid for the ſtomach; and ſome are contraindicated in many diſeaſes, for which we employ the bark in other menſtrua, or in ſubſtance. The nitrous acid is a very complete ſolvent of the reſin; but both it and the muriatic precipitate a gummy matter in the infuſions. The vitriolic acid is free from this objection; and the dulcified ſpirit of vitriol is an excellent aſſiſtant in preparing the cold infuſion. When the dulcified ſpirit of vitriol is properly prepared, it is leſs fit for this purpoſe than the liquor ſold by the druggiſts under that name; in which laſt the acid for the moſt part prevails. The following formula affords a very ſtrong infuſion of bark.

℞ Pulv. Cort. Peruv. ℥ſs. infunde in
Aq. moll. ℥vi.
Sp. Vitriol. dulc. ʒi. To ſtand twelve hours.

This infuſion has a very bitter and aſtringent taſte, accompanied with an agreeable acidity. If a piece of iron is put into it, the liquor ſoon acquires a black colour. It is certainly better to add vitriolic acid in this form, than to decoctions and infuſions already prepared.

From theſe experiments, I think, it will appear, that rectified ſpirit not only extracts the reſin of the bark, but alſo along with it a conſiderable part of its gum. This may probably take place from the mediation of the reſin, on the ſame principles as in watery menſtrua the reſin is ſuſpended chiefly by the aſſiſtance of the gum. The ſmell and taſte of the ſpirits drawn off, prove the exiſtence of an aroma in the bark, if any ſuch proof is neceſſary. The black colour of the reſin ſeems always to take place from the application of heat, either in a great degree, or long continued. It ſeems to carry along with it an exact criterion of the ſtrength of ſuch depoſits: For inſtance, the reſin, as depoſited from rectified ſpirit by affuſions of water, is of the exact colour of the bark, and is by far the ſtrongeſt; that depoſited from a firſt coction is next to the above in lightneſs of colour, and in ſtrength; that from repeated coctions, or long continued boiling, is darker than the preceding, and is much weaker; and, laſtly, that to which heat was applied in a dry form, was darkeſt coloured, and poſſeſſed no aſtringency at all.

I need not ſpeak particularly on my experiments with the reſinous depoſits of firſt decoctions. Reſpecting their ſolubility in rectified ſpirit and in water, they appeared to me not materially different from the reſin precipitated from rectified ſpirit.

The blackiſh coloured depoſits from repeated coctions were more ſoluble in water than the preceding, which rather ſurpriſed me; but theſe ſolutions were not ſo aſtringent, and, I think, contained more gummy matter than the proper reſin. I am hence led to ſuſpect, that the reſin is more volatile than the gum itſelf.

After theſe experiments were finiſhed, I began to examine the clear liquor from which all the precipitates had been taken. It had a bitter taſte, with a conſiderable degree of roughneſs. To two ounces of this liquor I added an equal quantity of lime-water. A pretty copious precipitation took place; but the mixture taſted conſiderably of lime-water; and, on breathing into the clear liquor, it let fall calcareous earth. This liquor produced the moſt beautiful green with a chalybeate of any preparation I have yet ſeen.

Having thus finiſhed theſe experiments, I next added the ſpirit I had drawn off, with as much water as reduced it to proof, to the reſiduum of the firſt tincture. This infuſion, after ſtanding ſeven days, became a very ſtrong good tincture, and was uſed as ſuch in ſucceſsful practice. This laſt circumſtance illuſtrates what was ſhown before, *viz.* that water can extract the virtues of the bark from the reſiduum of a tincture in rectified ſpirit.

As I had ſtill by me a conſiderable quantity of reſinous depoſits, and ſome of them pretty ſtrong, I was willing to try their effects on the ſtomach of living men. I therefore gave to a patient, who had been taking bark three days before, a tea ſpoonful of the reſinous matter which I obtained from the caput mortuum of the ſecond diſtillation. The doſe was taken in a bolus with conſerve of red roſes. It is proper to obſerve, that the patient's belly was a little looſe before; but in about two hours after taking this bolus, he was ſeized with very ſevere gripes, and had two or three looſe ſtools, and was likely going on to have more, had not the flux, for the ſecurity of my patient, been ſtopped by an opiate.

This trial was not, I own, altogether deciſive; but I think it amounts to a high degree of probability, that the purgative qualities of the bark are ſeated in its reſin, and in that more eſpecially when in a naked ſtate. I never again repeated this experiment.

From ſeveral very ingenious experiments, Dr Percival is led to ſuppoſe, that there ſubſiſts a particular affinity between acids, aſtringents, and bitters; and that they neutralize each other, forming what the chemiſts have called a *tertium quid*. In order to inveſtigate this matter as fully as I could, I took the advantage of Dr Percival's uſeful information, and repeated many of the trials on which the Doctor lays moſt weight to ſupport this opinion.

Before I ſet out with the following experiments, I think proper to acknowledge, that I do it with all imaginable diffidence.

As Dr Percival has ever been one of my favourite authors, from the beginning of my medical ſtudies to the preſent time, it is not ſurpriſing that I entered on this inquiry with every prepoſſeſſion in favour of the Doctor's opinion. I was even mortified when experiments obliged me to doubt the truth of an opinion for which I had often profeſſed myſelf an advocate in the company of my friends. I will however obſerve, that theſe experiments are nice and precarious, and many of them by no means deciſive. Whilſt therefore I relate my experiments, and freely mention my objections to this opinion, I muſt at the ſame time declare, that I neither believe that Dr Percival's experiments prove ſuch an union of acids and bitters, nor that my experiments fully prove the contrary.

EXPERIMENT XLI.

To two ounces of a ſtrong and freſh infuſion of red bark I added four drops of vitriolic acid. The taſte was at firſt, as I thought, completely neutral; but, on remaining in the mouth for ſome time, the bitter was evidently predominant, and more laſting.

EXPERIMENT XLII.

To two ounces of an infuſion of red bark I added a tea ſpoonful of the chalybeate. A deep purple or black was ſoon produced, and a copious ſediment fell to the bottom. On adding three drops of vitriolic acid, the original colour of the infuſion was perfectly reſtored.

EXPERIMENT XLIII.

To this liquor I added pieces of clean iron wire. After ſtanding for ſome time, the liquor became as black as before.

EXPERIMENT XLIV.

To the ſame quantity of infuſion and chalybeate, with three drops of vitriolic acid, I added a ſmall piece of zinc. After ſtanding for ſome time, the infuſion became firſt of a dark purple, and precipitated a black ſediment. The clear liquor gradually became green; the ſediment, however, remained black. It would appear from this laſt experiment, that zinc is capable of ſaturating the ſuperabundant acid, and thus regenerating the ink, but that the ſuperiority of its attraction for the acid is not ſufficient to diſcharge the colour entirely. I rather ſuſpect, that it ſeized very little of the acid of the martial vitriol, as I think this green colour was partly to be aſcribed to the age of the infuſion. The green colour did indeed appear in three days, which was in a much ſhorter time than I had obſerved that colour take place in the infuſions of the red bark.

EXPERIMENT XLV.

To two ounces of infuſion of bark I added four drops of vitriolic acid. The taſte, to my ſenſation, was exactly neutral. To this mixture I added ſmall pieces of iron wire, which very ſoon communicated a purple colour.

EXPERIMENT XLVI.

To two ounces of infuſion I added four drops of vitriolic acid. I alſo mixed the ſame quantities and proportions of water and acid. To each I added a quantity of zinc more than they could diſſolve. After ſtanding forty-eight hours, the infuſion and acid, with the zinc, weighed more than the ſame quantity of infuſion and acid alone, by two and a half grains. The water and acid, with zinc, weighed more than the ſame quantity of water and acid alone, by two grains, and ſome little over-weight, not amounting to a half grain. I am not able to detect any miſtake in theſe experiments; and I imagine that the thickneſs of the infuſion had in ſome meaſure prevented the free action of the acid on the zinc. The difference however was very trifling.

Theſe experiments render it probable, that the vitriolic acid is not ſo much changed from its union with bark, as to leſſen its action on zinc and iron *.

I tried the ſame experiment (without however weighing the liquors) with the terra aluminis, whoſe attraction for the vitriolic acid is ſo weak, that Mr Pott found it liable to be precipitated by zinc. In this trial alſo I found that the infuſion gained a manifeſt aluminous taſte.

I cannot help therefore entertaining much doubt concerning this opinion of Dr Percival's.

I preſume that no proofs of a neutral union of bodies can be admitted, except thoſe which clearly ſhow that ſuch is the combination of the two, that another ſubſtance known to have an attraction

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* The blackeſt ink that I remember of having ſeen, was in the poſſeſſion of Mr Maxwell of Broomholm. From the deepneſs of the colour, it is probable, that the martial vitriol and galls were here in very proper proportions. It was however evident, that, in this ink, the acid had by no means loſt its power of acting on iron, as Mr Maxwell found, that, in mending pens wetted with it, he ſeldom failed to blunt or deſtroy the edge of his knife.

tion for either, is not however capable of disjoining this union: But this, I think, is as yet by no means proved. All the ſubſtances we have employed have been acted on by the acid; which ought not to have happened, had its diſſolving power been leſſened by its union with the bark, as forming a neutral. Beſides, if the powers of each are increaſed by this union, and which Dr Percival has clearly ſhown, how ſhould we reconcile this to the general facts in chemiſtry, by which we know that a neutral union of bodies ſeldom fails to diminiſh the powers of each ingredient. As Dr Percival ſuppoſes that acids, when joined with bitters, loſe their property of changing the blue colours of vegetables into a red:

EXPERIMENT XLVII.

I mixed two ounces of infuſion with four drops of vitriolic acid and ſix drops of ſyrup of violets. The colour was ſo indiſtinct, that I could form no concluſion from this experiment. A gentleman, however, who ſtood by, and who did not know the intention of the experiment, ſaid, on being aſked, that he thought it was of a redder colour than a ſtandard of the ſame infuſion with the ſame quantity of ſyrup.

In the courſe of theſe experiments, I was filtering a ſolution of zinc in vitriolic acid; and, having no proper paper at hand, I accidentally made uſe of ſome common blue paper, which I found, on the firſt touch of the ſolution, inſtantly acquired a reddiſh colour.

After this diſcovery, I was willing to repeat the above experiment, and, as I expected, in a much more deciſive manner.

EXPERIMENT XLVIII.

To two ounces of infuſion I added four drops of vitriolic acid. I alſo mixed the ſame quantity and proportion of water and acid, and placed as a ſtandard the ſame quantity of a plain infuſion. In each of theſe liquors I immerſed a piece of blue paper, and took out the pieces after about five minutes. The piece in the plain infuſion was in no reſpect changed; that in the water and acid was ſlightly reddiſh; but, to my great ſurpriſe, the paper in the infuſion and acid was even redder than that in the water and acid. I imagined that I had made ſome miſtake, and therefore repeated the experiment immediately, but I ſtill met with the ſame reſult.

Why the piece of paper in the infuſion and acid was redder than that in the acid and water, when that in the plain infuſion was not in the leaſt changed,

ged, I am altogether unable to explain.

I cannot therefore agree with Dr Percival, that acids loſe their property of changing vegetable blues, by their union with bitters.

I will here obſerve a circumſtance, as connected with this ſubject, which, I preſume, has miſled Geoffroy. That uſeful chemiſt has aſſerted, that an infuſion of bark changes the blue colour of vegetables to a red; a proof, he thinks, of the acid he obtained from the bark by a chemical analyſis.

I am ſatisfied that Geoffroy had actually ſeen this; but I am alſo perſuaded, that he ſaw it in an old fermenting infuſion or decoction. I have frequently immerſed pieces of blue paper in freſh infuſions, but could never obſerve the ſmalleſt change of colour. I have however found this change take place in an old infuſion; and I aſcribe it entirely to the evolution of the fixed air.

Hence blue paper becomes a very eaſy teſt in judging of the age and ſtate of theſe preparations of the bark.

As Dr Percival has produced ſeveral very ingenious experiments to prove that acids and bitters, when combined, poſſeſs double the ſum of their antiſeptic powers when ſeparately employed, I was willing to make ſome experiments on this intereſting ſubject. I uſed the ſame proportion of vinegar with Dr Percival; but, by miſtake, employed a decoction of the red bark, which, no doubt, varied the proportions in the ſtrength of this mixture.

Without therefore relating the particulars of the experiments, I ſhall only remark, that, in all the mixtures wherein bark made a part, they kept longer ſweet than thoſe of Dr Percival's. For example, his decoction and mutton was frothy in ſeven hours. My decoction and mutton was not frothy till about twelve hours. My mixture with the decoction and vinegar was not putrid after twelve days. This laſt, however, was allowed to ſtand, for the laſt ten days, on the outſide of a window, in the month of September, and was broke by accident.

Theſe experiments not only convinced me of the juſtneſs of Dr Percival's opinion, but alſo of the ſuperior antiſeptic powers of the red bark. I did not therefore think it neceſſary to make any more trials than the following.

EXPERIMENT XLIX.

I divided two ounces of chopped mutton into equal halves, and put them into two different phials. To one I added four ounces of an infuſion of red bark. To the other I added four ounces of an infuſion of pale bark. The infuſions were of equal ſtrength and age. I put the mixtures into a ſmall oven, whoſe heat was kept as invariably as poſſible to ninety degrees of Fahrenheit's thermometer. That with the pale infuſion, or No. 1. emitted air bubbles in about nine hours time, and was a little frothy. That with the red infuſion, or No. 2. had ſome little inteſtine motion, but was not frothy at ſix of the clock next morning, though it had now ſtood about fourteen hours. No. 1. next morning was very frothy, but had no putrid ſmell. No. 2. was not frothy till nine of the clock. No. 1. had then a kind of muſty acid ſmell, like what we feel from an effervеſcing mixture. No. 2. was fermenting rapidly at noon. No. 1. was then evidently putrid, and ceaſed to emit bubbles. No. 2. continued fermenting till ſix of the clock in the evening, when it now began to have the ſmell of No. 1. at nine o'clock in the morning. No. 1. was now very fetid, and I threw it out. No. 2. had not ceaſed fermenting at ten o'clock at night: I did not however keep up the heat over night as before. It was given over fermenting next morning, but was not very putrid.

From theſe experiments it is obvious, that red bark preſerves animal matters much longer than the pale. It alſo appears, that, when the fermentation begins, it proceeds more rapidly, but is alſo longer continued with the red than with the pale bark: That the infuſions reſiſt fermentation longer than the decoctions of bark, though both reſiſt it conſiderably.

EXPERIMENT L.

Sir John Pringle tells us, that an infuſion of bark retards the fermentation of mixtures of fleſh and bread, but that an unſtrained decoction conſiderably accelerates that fermentation. But, from all my experiments, I was certain that both decoction and infuſion, whilſt freſh, conſiderably retarded fermentation, and the infuſion more ſo than the decoction. I was equally certain, that both, when old, did really accelerate that fermentation. I am therefore led to ſuſpect, that Dr Pringle had made uſe of an old decoction in its fermenting ſtate.

EXPERIMENT LI.

Dr Percival has aſſerted, that bark does not preſerve ox-gall, and that it coagulates this gall. Now, I have diverſified this experiment many ways, and with different watery preparations of bark; but I have always found that they tended leſs or more to preſerve the gall; neither have I found that the bark, in any form with water alone, was capable of coagulating ox-gall. I muſt therefore ſuſpect, that ſome miſtake has happened with Dr Percival.

From theſe experiments, I think, we may ſee the neceſſity of attending carefully to the different preparations, and even to the different ſtates of thoſe preparations, before we can determine the nature of the bark.

From the analogy between the decoction and old infuſion, it is, I think, obvious, that heat begins what age performs in a more perfect degree.

I think we may alſo find ſome analogy between the effects of a fermenting infuſion and thoſe of yeaſt. I am hence diſpoſed to think, that, by well conducted experiments, an artificial barm might be diſcovered. I have indeed made a few experiments towards an inquiry of this kind myſelf. But as I have hitherto been altogether unſucceſsful, I can furniſh no very uſeful information on the ſubject. I ſhall however obſerve, for the ſake of thoſe who may be engaged in the ſame purſuit, that what I have found moſt nearly reſembling barm, is a ſtrong infuſion of the heads of the white poppies, mixed with the yolks of eggs, and the whole ſlowly impregnated with fixed air.

As Dr Percival has made no experiments to determine whether the bark was changed from its union with the foſſil acids, I proceeded to the following trials, which I found required more nicety than any or perhaps all the experiments I have yet mentioned.

I cannot here enumerate the many diſappointments and difficulties I met with in the beginning of theſe experiments; but ſhall briefly relate their reſult, after repeated trials, and the matureſt improvement I could acquire in the art of making them *.

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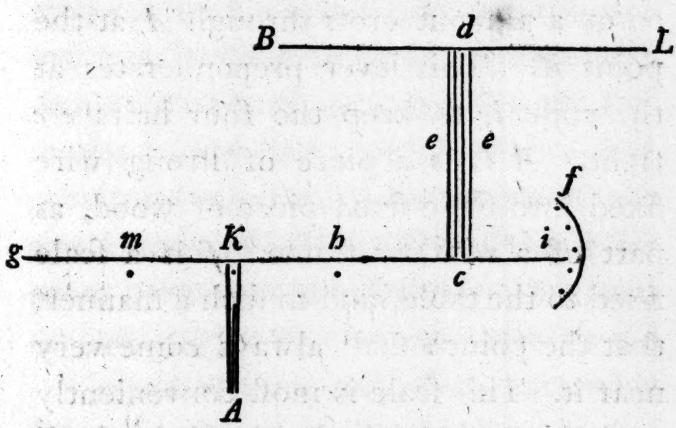
* It was ſuggeſted to me by a friend, that theſe experiments might fail of anſwering my purpoſe, as he imagined that animal matters were not contracted by the action of vegetable aſtringents. This opinion was plauſible, and was drawn from the known fact, that ſkins immerſed by tanners in infuſions of oak bark, acquire weight and thickneſs, at the ſame time that they retain their former dimenſions. My friend

The following is a ſketch of the inſtrument I employed.



A is a piece of thick wire, ſuch as clockmakers uſe for pendulums: It is ſawed

down

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friend had not however attended to the other proceſſes in the art of tanning. On inquiring into this buſineſs, I found, that, prior to the immerſion of the ſkins in the aſtringent infuſion, the tanner macerates his ſubject in a liquor produced from a mixture of barley and rye, malted, and ſet to fermentation. Others employ a ley drawn from pigeon's dung and ſuch like alkaline matters. The firſt, no doubt, affords a vinegar capable of ſoftening and opening the pores of the ſkin, ſo as to promote the free action, or perhaps to admit the particles of the aſtringent into its ſubſtance. Alkalis are known to have the ſame effect as vinegar; and both ſeem to enlarge the volume of the ſkin to a degree balancing the contraction to have been otherwiſe down a certain way through the middle at the point *K*. *g h* is a lever ſuſpended on a pin put croſs through *A* at the point *K*. This lever preponderates at the point *i*, to keep the four hairs *e e* tight. *B L* is a piece of ſtrong wire fixed into ſome fixed piece of wood, as part of a window frame. *f* is a ſcale fixed to the table, and in ſuch a manner, that the point *i* ſhall always come very near it. This ſcale is moſt conveniently made, by taking the head off the ſmall inſtrument we call a *gimblet*, and fitting to the ſharp end of it a piece of firm wood, cut afterwards and dreſſed, in order to make an equal ſurface and proper

wiſe expected from the action of the oak bark alone. From the acquiſition of weight, it is probable that theſe ſkins imbibe a conſiderable quantity of aſtringent matter. It is farther probable from hence, that ſhoemakers are able to communicate a deep black colour to the heels of their ſhoes by wetting them with a ſolution of martial vitriol. This, I apprehend, could not take place, but from the preſence of aſtringent matter in the ſubſtance of the leather. However theſe be, certain it is, that ſoft animal matter is evidently contracted by the action of vegetable aſtringents.

proper form. To this ſcale may be applied a piece of thin paper, by means of mucilage, or ſome ſuch adheſive matter. The ſcale is now fixed to the table by means of the other extremity of the gimblet, which is a kind of ſcrew, and is by that means more eaſily accommodated to the direction of the lever at the point *i*. The four hairs, *e e*, are woven at each end, in the manner barbers uſe for making wigs, and each extremity has a ſmall bulb. To employ this inſtrument, it is firſt fixed into the table; a ſmall piece of ſealing wax is melted upon the points *c* and *d*, and to which the bulbous extremities of the woven hairs are applied; the ſpot of the ſcale *f*, oppoſed to the point *i*, is then exactly marked: This done, the woven hairs are now moiſtened with the liquors to be examined; and, in proportion to their aſtringency, the point *i* riſes on the ſcale *f*, and the point *g* is proportionably depreſſed.

EXPE-

EXPERIMENT LII.

I moiſtened the hairs, *e e*, with a very ſtrong infuſion of red bark, and repeated the moiſtenings every twenty minutes. When it had ſtood three hours, I melted the wax at the points *c* and *d*, and took off the hairs, *e e*; I alſo marked the exact ſpot where the point *i* roſe to in the ſcale *f*.

EXPERIMENT LIII.

That the weight of the wax might not vary the reſult, I now applied the piece of thread at the point *m*, which balanced the lever with the firſt quantity of wax; and, on finding the wax a little lighter, I added more, till I reſtored the exact *æquilibrium:* This done, I ſoftened the wax by a diſtant heat, and applied other four hairs of exactly the ſame length as the preceding. Before I moiſtened theſe hairs, I gave a very gentle riſe to *B*, *L*, which brought the point *i* to that ſpot in the ſcale *f* to which the infuſion had raiſed it. I now moiſtened the hairs with vitriolic acid and water, in the proportion of four drops of the former to two ounces of the latter. I repeated the moiſtenings, and allowed the length of time as before mentioned. I now took off theſe hairs alſo, as before, after having marked the exact ſpot on the ſcale *f* to which the vitriolic acid and water had raiſed the point *i*.

EXPERIMENT LIV.

I again applied the piece of thread to the point *m*, and balanced the lever as before. This done, I again applied freſh hairs, and, by gently depreſſing *B L*, brought the point *i* down to the point on the ſcale *f* where it ſtood when I firſt applied the ſimple infuſion. I now moiſtened the hairs with infuſion and acid, in the ſame proportions I had uſed of the water and acid. I repeated the moiſtenings, and allowed the length of time before mentioned. I perceived that the infuſion and acid was quicker in its effect than either of the preceding; I however allowed it to ſtand the ſame length of time, and marked the exact ſpot on the ſcale *f* to which it raiſed the point *i*. My reſults were the following. To be more clear, I ſhall call the ſimple infuſion *A*, the water and acid *B*, and the infuſion and acid *C*.

A roſe a conſiderable length, I think nearly the twentieth part of an inch; *B* roſe, I think, ſome more than the half of that length; *C* roſe (as near as I could gueſs) as much above *A* and *B*, as *B* was raiſed above *A*.

It hence appears, that *C*, or the infuſion and acid conjoined, is more powerful than *A* and *B*, or the infuſion and acid ſeparately employed, by as much as *A*, *A*, or the infuſion, is more powerful than *B*, or the water and acid.

* We cannot reduce theſe riſings to any exact menſuration; but one may really judge pretty nearly by the eye. I am however convinced from theſe experiments, that the vitriolic acid and bark conjoined are much more aſtringent than when ſeparately employed. It might be here ſaid, that vitriolic acid ſhould on this account diminiſh the purgative property of the bark; but the contrary of this is very frequently obſerved. From this circumſtance, and the trial I made with the naked reſin on a patient I formerly mentioned, I am led to ſuſpect, that the operation of

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* On mentioning the inconveniencies of this inſtrument to my ingenious friend Mr William Keir in Milnholm, he gave me a plan of a lever which moved in a horizontal direction, and always ſteadily. I cannot here give a draught of this inſtrument; but I am fully perſuaded, that, from the mathematical exactneſs with which it is conſtructed, it would be found adequate to the execution of the niceſt experiments.

aſtringents is not an abſolute power with reſpect to the living animal body. Their effects are, I apprehend, in ſome meaſure, relative to their own ſtate, and to the ſtate of the body with reſpect to ſenſibility and irritability. I remember a patient in the clinical ward of the Royal Infirmary of Edinburgh, who was briſkly purged by large doſes of alum, preſcribed by Dr Gregory. I would even conjecture, that, in irritable bowels, three or four drachms of alum would purge, though half a drachm of the ſame alum would aſtringe: But this experiment I never had an opportunity to try. With reſpect to the bark, in particular, nothing is more obvious, than that its effects are often relative to the ſtate of the body at the time. It is certainly an aſtringent in the greateſt number of inſtances, and it has cured diarrhœa. It frequently, however, produces looſe ſtools, and theſe very copious. When thrown into a tolerably ſound ſtomach, it whets the appetite, and binds the belly. When thrown into the ſtomach of a perſon under fever, or diſeaſes of general debility, it often produces nauſea, and is rejected by vomiting, or is diſcharged by looſe ſtools. In fevers accompanied with the phlogiſtic diatheſis, the bark has been generally ſuppoſed to augment the heat of the body, harden the pulſe, and bind the ſkin and kidneys. In fevers attended with much debility and irritability, it is found to allay heat, to ſoften the pulſe, and promote perſpiration and urine.

I may perhaps be blamed that I did not carry theſe experiments with the lever conſiderably farther. But whoever tries this method, will ſoon be convinced, that it is a very nice, tedious, and difficult proceſs. When it was firſt ſuggeſted to me, in the fifth volume of the Medical Eſſays, I was very ſanguine in my expectations, that I ſhould be able to make out a ſcale of the comparative aſtringent power of different barks, and their preparations, with a very uſeful degree of exactneſs. But, on trying that method, (which, by the by, was firſt propoſed by Dr Hales), I was always foiled. It was not indeed till after material improvements in the conſtruction of the inſtrument, and a dexterity acquired in uſing it, that I could conduct the experiments with any tolerable degree of certainty. The hairs ſhould all be taken from the ſame head, and ſhould be as long as they can be procured. I have found ſome hairs ſo defended by an oleaginous covering, that they were not to be acted upon by watery liquors. The weight, moiſture, and other ſtates of the atmoſphere, may alſo, I think, vary the reſult. Four hairs anſwer better than one, as the united contractions raiſe a greater weight. I ſuſpect, that, when hairs are fixed by a knot, the contraction will be partly loſt at the fixed point. However that be, the riſings of the lever were not nearly ſo evident in this way, as when I employed ſealing wax and four hairs.

I next proceeded to try the effects of a mixture of bark and vinegar on dead animal matter.

EXPERIMENT LV.

Two pieces of freſh ſheeps ſkin were immerſed, one in an ounce and a half of an infuſion of red bark, the other in the ſame quantity of infuſion with two drachms of vinegar. After they had ſtood about fifteen or ſixteen hours, I took them both out, and could not perceive any ſenſible difference. Both were conſiderably ſhrivelled. A quantity of brown ſediment was depoſited from the infuſion with vinegar. I again immerſed them each in their former liquors; and, after allowing them to ſtand three days, the piece that was immerſed in the infuſion with vinegar was leſs ſhrivelled than that in the plain infuſion. I muſt therefore ſuſpect, that Dr Percival has been in ſome meaſure deceived in making this experiment. Dr Percival himſelf obſerves, that vinegar always depoſited from the infuſions a large quantity of a brown ſediment. It is reaſonable then to expect, that the mixture would ſuffer a conſiderable diminution of its aſtringency from the precipitation of this matter; nor can we think that the ſediment could contribute much to ſhrivel the ſkin, when it now ceaſed to be in a ſtate of ſolution.

I regret that I had not made more experiments to this purpoſe. I think, however, the above render it at leaſt probable, that the aſtringency of the bark is not diminiſhed by vinegar, farther than an admixture of that acid is a counterpoiſe to the conſtringing powers of the bark. We may alſo obſerve, by the way, that, as vinegar precipitates the matter of the bark, it is not probable that the two form any neutral union. If Dr Percival has concluded that they neutralize each other from the change of taſte that ſuch a combination produces, I imagine that, on this principle, we might diſcover a great many neutrals which have not hitherto been thought of. We however obſerve, that acids and bitters are particularly ſuited for diſguiſing each others taſte. For inſtance, Glauber's ſalt is known to have a very diſagreeable and bitter taſte; but I was firſt taught by Dr Gregory, that this taſte is very much covered by the addition of an acid. It is however obvious, that, inſtead of any neutralization being here performed, we render our ſolution ſo much the more diſtant from a neutral as we add the more acid.

As, in the courſe of theſe experiments, I had frequent occaſion to obſerve very different appearances in the production of ink from the different preparations and ſtates of the bark, I made a few experiments with a view of inveſtigating this matter more fully.

EXPERIMENT LVI.

After making a pretty deep coloured ink with an infuſion of red bark and a chalybeate, I regenerated the clear colour with vitriolic acid. I next ſuperſaturated the acid with ol. tartari per deliquium: The colour was now conſiderably blacker than the original ink.

EXPERIMENT LVII.

I repeated the ſame experiment with fixed cauſtic alkali; but the colour was fainter than the original ink. The ſediment was of a ruſty hue.

EXPERIMENT LVIII.

I tried the ſame experiment with the earth of alum. It firſt produced the original colour; but when the acid ſeemed to be ſaturated with it, the colour became more faint.

EXPERIMENT LIX.

As I imagined, that, in Experiment LVII. I might have added too much alkali, I repeated the experiment often. But in no quantity that I could add, from one drop to half a drachm, was I able to produce an ink blacker, or I think ſo black, as the original.

EXPERIMENT LX.

I tried this experiment alſo with common magneſia, and the reſult was that of Experiment LVI.

EXPERIMENT LXI.

I repeated the ſame experiment with calcined magneſia, and the reſult was that of Experiment LVII.

EXPERIMENT LXII.

I likewiſe made this experiment with plain chalk, and alſo with chalk reduced to quicklime. With the former I had the reſult of Experiment LVI.; with the latter, that of Experiment LVII.

From theſe experiments I was led to think, that the production of ink was in ſome manner connected with the preſence and ſtate of the fixed air of theſe bodies. It is obſervable, that, on the firſt addition of a mild alkali or calcareous earth to ink thus made clear by an acid, an efferveſcence takes place; but the moment that the black colour begins to appear, the efferveſcence ceaſes, and the ink becomes blacker and blacker, depoſiting a great quantity of ſediment. That this efferveſcence happens from the ſuperabundant acid only, is clear, from no ſuch efferveſcence taking place on the addition of alkali to the original ink.

EXPERIMENT LXIII.

To two ounces of infuſion I added a tea ſpoonful of my chalybeate. After ſome time, a very deep purple was produced. I put the ſame quantity and proportions of each into another glaſs. To the one I added two ounces of water impregnated with fixed air: I diluted the other with an equal quantity of common water. That with the * mephitic water was blacker than the other diluted with the ſame quantity of plain water.

Experiment LXIV.

Being allured by theſe experiments, I was willing to go ſome ſteps farther, in order to diſcover a method of recovering the legibility of writ rendered dim by age. Having therefore moiſtened a piece of paper containing ſome very dim writ, I put it into a jar containing fixed air. I could not however obſerve any material difference in its colour, though it ſtood in the jar three or four hours.

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* I muſt however obſerve, that, on repeating theſe experiments ſome time after, I was not ſo fully ſatisfied of this one with the mephitic water; and I can very well remember, that the water I then employed was become very weak by age. I am at the ſame time certain, that, at that time, it produced the effect mentioned.

EXPERIMENT LXV.

I next included part of the ſame writ in a jar containing alkaline air, as ſeparated from the cauſtic ſpirit of ſal ammoniac. In this experiment alſo I could obſerve very little difference: If there was any, the writ was really dimmer.

EXPERIMENT LXVI.

The writ had an ochery colour; from which imagining that the alkaline gas would be more effectual when applied to it in the ſtate of ſolution, I therefore put the writ into a jar containing marine acid air. By this gas the colour was ſenſibly diminiſhed.

EXPERIMENT LXVII.

I now put the piece of writ of Experiment LXVI. into a jar containing alkaline air ſeparated from ſpirit of hartſhorn. The colour was now recovered; but I could not be certain that the writ was blacker than before the application of the marine acid air.

EXPERIMENT LXVIII.

I held a piece of moiſt paper, containing very dim writ, over a pan of boiling ſolution of liver of ſulphur. The writ acquired ſtill more of an orange hue.

I thought it unneceſſary to try Experiment LXVI. with vitriolic acid air, as Dr Prieſtley tells us, that that gas is incapable of diſſolving iron.

My reaſon for making uſe of the mild volatile alkali in Experiment LXVII. was, that I might with the alkaline alſo ſeparate ſome fixed air.

I confeſs myſelf very much at a loſs

to give any rational account of the reſult of the preceding experiments.

The firſt circumſtance that ſtrikes us is, that the precipitate formed by the mild alkali was more copious than that produced by the cauſtic ley. This might perhaps be accounted for from other well known facts in chemiſtry. We know that fixed air is capable of combining with metallic precipitates; and that alkalis, when deprived of this air, are in ſome inſtances unable to precipitate metals from their acid menſtrua. For example, in a ſolution of ſilver in the nitrous acid, this metal cannot be precipitated by the cauſtic ſpirit of ſal ammoniac, but is eaſily done by the mild volatile alkali. From the experiments of Mr Hey of Leeds, we are even informed, that fixed air, of itſelf, is able to precipitate lead from its ſolution in vinegar.

It may alſo be proper to obſerve, that alkalis are capable of diſſolving iron, and that the colour in Experiment LVII. very much reſembled that of the *martial alkaline* tincture of Stahl.

Theſe facts may, I think, make out ſomething for the explanation of the preceding experiments.

If we ſuppoſe, then, that this precipitation of iron is more readily effected by mild than cauſtic alkalis, we next find ourſelves puzzled to account for that fact. We muſt here again have recourſe to the analogy of other phænomena in chemiſtry.

It ſeems pretty clearly proved by experiment, that air and phlogiſton reciprocally precipitate each other. Thus, when metals are deprived of their phlogiſton by heat, their calces are found to have abſorbed a large quantity of air. When again theſe calces are reducing to a metallic ſtate, an * elaſtic fluid is obſerved to eſcape, and they are known to ſuſtain a conſiderable loſs of weight during the operation.

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* Some doubts have been entertained concerning the nature of this air. It ſeems, however, evident, that it is of the

From theſe united facts, I would deduce the following conjectures. That this precipitation of iron by a mild alkali is an inſtance of double elective attraction: That the alkali ſeizes on the acid, whilſt the calx ſeizes on the fixed air of the alkali: And, next, that the greater blackneſs of the colour is owing to a precipitation of the phlogiſton of the aſtringent, in conſequence of the eſcape and abſorption of the fixed air. It may appear inconſiſtent that I ſhould aſcribe the blackneſs of this precipitate

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the ſame kind as that ſeparated from mild alkalis and calcareous earths.

It is well known, that metallic calces have a powerful attraction for this kind of air, and are even able to render mild alkalis cauſtic. We have no ſuch proof of their attraction for pure air, and they have not been found to afford this kind of air, but when the calx was reduced *per ſe*; in which inſtance, there appears to happen ſome decompoſition of the original fixed air during the revivification of the metal.

to the precipitation of phlogiſton, when, according to my former reaſoning, this could not happen, now that the calx is charged with the fixed air of the alkali. It may however be ſuppoſed, that the calx * is not ſo much charged with the fixed air of the alkali as to preclude the precipitation of phlogiſton in a looſe ſtate, a ſtate moſt ſuitable to the production of a black colour. That this is the caſe, may be inferred from the total deſtruction of this colour by a farther addition

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* I may here obſerve a circumſtance which, I own, ſurpriſed me a good deal, but which I have not hitherto been able to aſcertain with abſolute certainty and preciſion.

On mixing mild alkalis with theſe inks, I imagined that the ſum of the two liquors thus conjoined was leſs than the ſum of them ſeparately. What I am more certain of, was, that, on mixing theſe liquors, a very beautiful undulatory motion, accompanied with a centrifugal appearance at the ſurface of the liquor, was exhibited. If the firſt circumſtance did really take place, I acknowledge myſelf altogether unable to account for it, or even to form any analogy between it and ſimilar phænomena that happen on the mixture of alcohol and water, of vitriolic ammoniac and alum, or, where it is ſtill more evidently obſerved, in the mixture of nitrous and empyreal airs.

dition of alkali. I would alſo obſerve, that alkalis do not precipitate the metal in a black form, except when conjoined with vegetable aſtringents: And, further, it is improbable that this phlogiſton is derived from the alkali, as phlogiſticated alkali forms Pruſſian blue; a matter wholly inſoluble in acids, and therefore widely different from the precipitate of ink.

With reſpect to the effects of the cauſtic alkalis, the firſt queſtion that naturally occurs is, Why does it leſs readily precipitate iron from its ſolution in acids? I would conjecture that the anſwer to this might be, Becauſe the cauſtic alkali cannot furniſh this air to the precipitating metal, and thereby give it a calciform nature; and farther, that, as the precipitate is not rendered leſs ſoluble by a ſupply of air from the alkali, the precipitate is therefore eaſier and ſooner diſſolved by this alkali forming the martial alkaline tincture of

Stahl. In endeavouring to account for the increaſed blackneſs by the addition of the mild alkali, the reverſe of the circumſtances there taking place, is my attempt to explain the defect of that colour in the fifty-ſeventh experiment. It is not probable that the want of this black colour was owing to any immediate ſolution of the iron by the alkali, as calcined magneſia and quicklime exhibited the ſame phænomena, though both of them are unable to diſſolve iron.

Having thus attempted to explain the action of alkalis on * bark and martial vitriol conjoined, it remains that we ſhould take notice of the effects of bark and

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* It may here be proper to obſerve, that theſe experiments are not applicable to ink made with galls, as in that caſe a much ſmaller quantity of alkali is ſufficient to diſcharge the colour. From this it farther appears, that the mild alkali ſupplies that principle in the bark, by the defect of which bark is leſs fitted to form ink than galls. The latter are not only more aſtringent, but are alſo known to contain a ſaline matter ready-formed in their ſubſtance.

and other vegetable aſtringents in the production of ink. I apprehend, however, that this is a much more difficult taſk than the preceding.

Every one knows, that bark and alkalis are very different in their ſenſible qualities. We have however ſeen, that, in their effects on chalybeates, they are very much analogous. It appears at firſt ſight, that the iron is charged with ſomething from the aſtringent, which forthwith renders it no longer ſoluble in the vitriolic acid. It is alſo natural to think that this matter is phlogiſton. We however know, that alkalis ſaturated with phlogiſton precipitate from iron a matter quite different from the precipitate of ink. We muſt at the ſame time obſerve, that the phlogiſton in alkalis, and in vegetable aſtringents, ſeems to be in a very different ſtate. But as it would be foreign to my ſubject to enter fully into this inquiry, I ſhall only obſerve, that the effects of the mild-alkali in heightening the colour of ink, and the effects of vegetable aſtringents in producing it, ſeem really to maintain a very ſtriking analogy. I may alſo remark, that, however diſſimilar alkalis and vegetable aſtringents may be in their ſenſible qualities, certain it is, that theſe aſtringents afford, on burning, a very large quantity of fixed alkaline ſalt.

From the preceding experiments, I think it will appear, that fixed air is an eſſential requiſite in the production of ink. I likewiſe ſuppoſe, that this air is only neceſſary as it is ſubſervient to the depoſition of phlogiſtic matter in a particular ſtate and form.

I ſhall conclude with a few remarks illuſtrative of this opinion.

Almoſt all good inks are pale when firſt applied to the paper; but, in the courſe of drying, their colour becomes much blacker. This, I apprehend, may be owing to the abſorption of fixed air from the atmoſphere, as drying the writ haſtily at the fire prevents this improvement of the colour. It is a common practice with ſome people to breathe on their writ, in order to haſten the approach of the colour: This, I apprehend, may be explained from the depoſition of fixed air from the lungs.

I have alſo often thought, that the black colour which takes place on the exterior ſurface of the fæces of a dog by expoſure to the air, was to be explained on ſimilar principles. And, laſtly, many of the black dyes, ſuch as the juice of the anacardium, are at firſt of a reddiſh brown hue, and gradually become black by expoſure to the external air.

I have thus finiſhed my experiments on the red and quill Peruvian barks.

I have not thought it neceſſary to relate many experiments made from preconceived opinions, in which I was often diſappointed and miſtaken. Every one acquainted with the buſineſs muſt be ſenſible, that ſuch a detail would have ſwelled this volume to an indiſcreet bulk; nor can I think that the public could be at all entertained with a relation of my blunders. I next proceed to offer

SOME THOUGHTS
ON THE
MODE OF OPERATION
OF THE
PERUVIAN BARK.

I DO not propoſe entering upon any full inquiry into this ſubject; and what I ſhall ſay here is principally intended as an application of the preceding experiments and obſervations. I apprehend that it would now be too late in the world to attempt any ſerious diſcuſſion of many doctrines on this intricate ſubject. Some of them are indeed fraught with abſurdities ſufficiently glaring to reflect their own refutation. It is obvious, that theſe opinions muſt have been equal in number and variety to the ſuppoſed cauſes of intermittents.

The firſt moſt plauſible and generally received opinion of the proximate cauſe of intermittent fevers, ſeems to have been that adopted by Dr Sydenham, and which may be learnt from the reaſoning * he employs to illuſtrate the impropriety of giving the bark immediately prior to the acceſſion of the fit.

After reading this paſſage, we would expect to find that Dr Sydenham conſidered the operation of the bark as an aſtringent power. But he thought otherwiſe; and, when treating of the gout, he goes about to prove the fallacy of truſting to the dogmatic plan, by adducing, as an inſtance, the efficacy of the Peruvian bark in the cure of intermittents. He

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* Cum enim vis ingens materiæ febrilis, diebus a paroxyſmo vacuis, in corpore fuit adgeſta, pulvis prædictus, ſi immediate ante paroxyſmum deglutiatur, facit, quo minus materia ipſa naturæ methodo, paroxyſmi ſcilicet impetu, poſſit eliminari.

He therefore reſts ſatisfied that the bark is a ſpecific remedy.

Dr Sydenham ſeems to have been drawn into this concluſion, partly by his attachment to the ancient fabric of morbific matter, and partly by the different effects of the bark in the cure of fever. From its exhibition being frequently accompanied with diarrhœa, he heſitated to pronounce its effects to depend on any aſtringent power. But finding its operation more generally attended with vigorous appetite and a bound belly, he could not impute to it any power of evacuating his *materia febrilis*. Puzzled with appearances ſeemingly contradictory, and which the ſtate of pathology at that time could not eaſily reconcile, he was willing to call it a ſpecific, which, with a man of his liberal inquiry, implied no more than that he cautiouſly ſuſpended his opinion on the ſubject, leaving it to the future examination of poſterity.

It were to be wiſhed that thoſe who adopted the theory of Dr Sydenham, had alſo cultivated the ſame induſtrious plan of obſervation and freedom of opinion; but many authors after him truſted more to his theory than he did himſelf; and that caution which in the character of Sydenham will ever do honour to his memory, degenerated in others to a ſuperſtitious timidity and unmeaning tales.

An opinion leſs generally received, but, I preſume, not leſs plauſible, was that of Dr Morton. After refuting numberleſs idle hypotheſes, he advances one of his own, which, though not ſatisfactory, is however ingenious, and, I alledge, may have been amongſt the firſt dawnings of a reaſonable pathology of fever. It is evident, that he conſidered the bark as a ſpecific * remedy; but he went

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* At ſane cortex febriles fermentationes, neque facultate obſtruente, neque deobſtruente, ſiſtit, verum vi ſua alexipharmica, qua eximie præpollet.

went a ſtep farther, and alledged that we are to ſeek for the cauſe of fever, and the operation of the bark, in the *impetum faciens* of Hippocrates, which, he at the ſame time maintains, is beyond the ſphere of our knowledge. It is however probable, that he conſidered the medium of the *impetum faciens* as a fluid, which, in theſe days, and long after, had the name of animal ſpirits. It is obvious, then, that Dr Morton placed the cauſe of intermittents in the nervous ſyſtem, though his notions of that ſyſtem were too imperfect to be eaſily reconciled with modern diſcoveries and reaſoning.

Franciſcus Torti has taken great pains to adduce and refute numberleſs very ſingular and abſurd opinions on this ſubject. In thoſe times, phyſicians had ample latitude to diſplay the fertility of their imagination where any hypotheſis, however extravagant, was not eaſily refuted. It might have been expected, that Torti, who has ſo fully examined the opinions and ſhowed ſo much reſpect to the writings of Dr Morton, would alſo have ſcreened himſelf, by ſuppoſing that the effects of the bark were a hidden operation on a hidden power. After however making ſome indeciſive experiments with bark *extra corpus* on the bile and other fluids, we find him ſtill ſeeking his *cauſa febrilis* in the blood, ſuppoſing it to be *exiguæ molis*, and that the bark has as it were an elective attraction for this *cauſa febrilis*, correcting it as an acid does an alkali, and forthwith expelling it from the habit. It appears, then, that Torti was a little more bold than Sydenham; and being determined to aſſign ſome cauſe, he fixed on one which many believed, though few could underſtand, and ſtill fewer could demonſtrate to be true or falſe. Fettered in the trammels of morbific matter, his reaſoning on this ſubject is in many parts ſo learnedly obſcure, that, if we were not acquainted with his accuracy in other matters, we ſhould have ſuppoſed him to be an alchemiſt.

Whilſt phyſicians were ſolely engaged in ſtudying the complicated wars betwixt nature and a morbific matter, and when they judged it their main buſineſs to ſtand firm, and ſee fair play betwixt the combatants, it was not probable that great diſcoveries were to be made in the therapeutic department of the healing art. Theſe, however, like many hypotheſes of a like nature, ſeem to be faſt paſſing away, and in this enlightened age will probably

"diſſolve,
And, like the baſeleſs fabric of a viſion,
Leave not a wreck behind."

The theories of Boerhaave and Hoffman did not differ much regarding the uſe of the bark in intermittents. We find however the learned commentator of Dr Boerhaave fairly diſſenting from the opinion of his maſter, though he affects the contrary. He has indeed produced full evidence, from the ſudden attacks of intermittents, and from many other circumſtances, that the proximate cauſe is not to be looked for in our fluids. It is indeed to be acknowledged, that Dr Boerhaave placed part of his proximate cauſe in what he calls the *liquidum nervoſum* *. This however is at beſt a *petitio principii*, which being granted, would only prove that a lentor of ſuch a fluid is altogether incompatible with life.

We ſhall however leave theſe traces into the opinions of our anceſtors, and take a ſhort view of the ideas of the principal modern pathologiſts on this ſubject.

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* It was not till after ſeveral editions that this alteration appeared in the works of Dr Boerhaave; and it is with probability ſuppoſed, that this great ſyſtematic made this improvement in the aphoriſm, on the particular inſtigation and advice of his nephew Kaw Boerhaave and the Baron Van Swieten.

Though Sir John Pringle was diſpoſed to think, from his experiments, that the operation of the bark was an antiſeptic power, yet he candidly owns, that ſuch is its quick operation in the cure of fevers, that we could not impute it to any action on the fluids. Dr M'Bride found alſo from his experiments, that the bark was highly antiſeptic, and ſeems to have laid conſiderable ſtreſs on its effects in this way. Yet he ſeems aware of ſeveral objections to this opinion, and draws many of its advantages from its aſtringent power, by which he thinks it indirectly antiſeptic, by increaſing the coheſion of the animal fibres, and preventing the eſcape of their fixed air.

There are indeed many reaſons for alledging that the bark does not at all enter the maſs of blood. Dr Alſton has known it thrown up unaltered many hours after taking it. Allowing that it does really enter the lacteals, it appears from De Haen's calculations, that, when diffuſed over the whole maſs of fluids, its quantity at any particular part muſt be ſo exceedingly ſmall, as not to be able to effect thoſe remarkable changes which we daily obſerve it produce in gangrenes and ſimilar diſeaſes. We may alſo remark, that Dr Friend found, that, by injecting bark into the veins of a dog, the animal was preſently killed.

I know it will be objected to this experiment, that milk, which is acknowledged to be a mild fluid, produces the ſame effect. We cannot however judge from the ſenſations on the tongue, what may be the effect of bodies applied to the internal ſurface of our blood-veſſels; nor is it at all probable, that entire milk enters the lacteals of animals.

Some have adduced as a proof that bark enters the blood, that, when adminiſtered in the form of injections into the anus, it has effectually cured intermittents. I muſt however ſuſpect, that this practice is by no means ſo ſucceſsful as that of throwing it into the ſtomach; and, allowing that it were, I preſume we may refer its effects in that form to the ſympathy that ſubſiſts betwixt the ſtomach, the bowels, and the ſkin. We cannot otherwiſe account for the ſudden effects of an opiate clyſter in the removal of vomiting.

I ſhall now take the opinion of thoſe who, I think, have thought moſt reaſonably on this ſubject.

I would therefore, with them, ſuppoſe that * "the effects of bark do not ariſe from its operating on the fluids, and muſt therefore depend on its operating on the nerves of the ſtomach, and being thereby communicated to the reſt of the nervous ſyſtem; that its operation is a tonic power, the bark being a remedy in many caſes of debility."

If we conſider the different principles in the bark, and ſurvey each of them apart, we ſhall ſoon find, that a combination of theſe principles, and therefore bark, is wonderfully ſuited to poſſeſs a tonic power.

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* Dr Cullen.

I think Dr Percival and other writers have clearly ſhown, that bark is a mixture of aſtringent, bitter, and aromatic parts. Let us therefore take a view of theſe ſeparately in their effects on the living animal body.

* " The changes induced in the ſyſtem from the primary effects of aſtringents, are, an increaſe of the power of coheſion in various parts of the animal body ; an increaſe of what may be termed the tonic power in the ſyſtem ; a diminution in the capacity of containing veſſels in the ſyſtem ; a diminution of irritability, and perhaps in ſome degree of ſenſibility."

Bitters, I preſume, poſſeſs powers in common with aſtringents and aromatics conjoined, I mean with reſpect to the living animal body.

If we conſider the aroma of the bark as a ſtimulant, which it moſt probably is,

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* Dr Duncan.

is, we know, that the * "changes induced in the ſyſtem from the primary effects of ſtimulants, are, acceleration of the motion of the blood in the part to which they are particularly applied; an increaſe of the force of circulation in general; an increaſe of excitement in the powers of ſenſation; and an augmentation of mobility and vigour in the muſcular fibres through the ſyſtem in general."

From comparing the effects of theſe ſeparately, with what may be expected from a proper combination, I imagine that every one will ſuppoſe, that an union of the three ſhall be beſt ſuited to diſeaſes of debility.

We find, that the bark, and all other bitters, on long continued uſe, produce ſedative effects on the ſtomach, and therefore on the ſyſtem. Now, I would conjecture, that the aroma ſerves the ſame end as the living principle (though in

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* Dr Duncan.

in a leſs degree), in preventing the aſtringent parts from acting on the living ſtomach, as they would do on dead animal matter.

I do not ſuppoſe that the aroma alters the inherent properties of the aſtringent part; but I imagine that it effects this end, by exciting and increaſing the ſenſibility of the ſtomach, or, in the words of our quotation, that it produces an " increaſe of excitement in the powers of ſenſation." The converſe of this may alſo be conceived, *viz.* That the aſtringent moderates the influence of the ſtimulating power, by an exceſs of which the bark might have been leſs permanent in its effects, or have been followed by a degree of collapſe exceeding that of the original excitement. It is indeed ſurpriſing what quantities of bark ſome patients will take with impunity. De Haen tells us of a man who, in the courſe of ſeven months and a half, conſumed fifty-two pounds and ſeven ounces of bark: A great part, however, was employed in the form of fomentation to ſores with which he was afflicted.

As a proof that the ſuperior effects of bark ariſes from a combination of theſe different principles, it has been always moſt ſucceſsfully imitated by a mixture of the three.

It might be here aſked, ſince we are thus acquainted with the principles of the Peruvian bark, Why are we not able to make a combination of theſe principles invariably ſerve us as a ſubſtitute adequate in power to the bark itſelf? I apprehend that the anſwer to this is ſhort and ſatisfactory: In ſome caſual inſtances, we hit on ſuch a ſubſtitute as anſwers our purpoſe; but, in general, no human device is equal to the laboratory of nature.

I would alſo obſerve, that though bark, as a mixt, ſeems in moſt part of caſes to be admirably well proportioned for the cure of diſeaſes, yet we have frequent inſtances of its want of ſucceſs, and ſeemingly owing to a defect of ſome or redundance of others of theſe principles. Thus we have frequent occaſion to obſerve, that bark joined with aromatics * often ſucceeds, when entire bark has little obvious effect. Bark has alſo ſucceeded after the uſe of wine †, when it had no good effects before. It has alſo ſucceeded when joined with ſaline ſtimulants ‡, when ſeparately taken it produced no advantage. ‖ Nay, this fact

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* Hoffman tells us, that, in phlegmatic patients, bark is much aſſiſted by caſcarilla.

† Dr Home relates a caſe of a man who could not be cured of gangrene by bark, till after he had drunk pretty freely of red Port.

‡ Dr Webſter was able to cure intermittents by bark and ſal ammoniac conjoined, when bark ſeparately taken was unſucceſsful.

‖ I once attended a patient under remittent fever, along with Dr Clapperton of Lochmaeban, where the bark had no obvious effect till joined with Dover's powder.

fact was obſerved by Morton; and he employed what in thoſe days were called cardiacs and alexipharmics, prior to or in conjunction with the bark.

I cannot here enter into the rationale of theſe facts; but I think they would not be very difficult to explain. I ſhall conclude with obſerving, that this relative operation of the bark does not appear to have been ever much attended to. The general notion of the bark being a remedy whoſe operation was altogether inexplicable, has, we preſume, contributed much to this neglect. It is however allowable to examine into this matter; and it is certainly not the buſineſs of ſcience to ſtand appalled at ſuch pretended ſecrets. Let us therefore conſider the operation of the bark no longer as ſuch; and let us vary the proportion of its principles, as we find them indicated by the ſtate of our patient. Experience alone can fully determine the propriety of ſuch a practice; but I muſt ſuſpect, that many failures in the uſe of bark may be prevented by a prudent change of the proportions of aſtringent, bitter, and aromatic parts, of all which I conſider it as a mixt.

I think it is obvious, that the antiſeptic power of the bark is an effect of its tonic power only; hence, I preſume, it is very properly ranked among the *antiſeptica tonica* *.

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* *Vide* Dr Duncan's Therapeutics.

ON THE

USES OF THE BARK.

TO give a full account of the uſes of a remedy ſo extenſively applicable as the Peruvian bark, would far exceed the limits allotted to this diſſertation.

Every one knows, that, on its firſt introduction into Europe, many celebrated phyſicians condemned its uſe altogether, and many long after ſpoke of it as a very ambiguous and dangerous remedy. Some of thoſe ſeem to have been more attached to theory, and the firſt opinion they had taken up, than to make any accurate examination of the ſubject. Nay, we find many of them * paſſionate and very ill natured when ſpeaking of the Peruvian bark. We

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* Romæ ſcribo, et in aere Romano, et ideo *garriant* quicquid velint chinæ chinæ fautores; aliis forſan in urbibus egregium eſt remedium, hic noxum experior.

BAGLIVII Opera.

cannot therefore expect any unbiaſſed obſervations from the writings of men ſo diſpoſed.

What in Sydenham was probably a miſtake in relation to the caſes of Underwood and Potter, ſeems to have miſled many phyſicians after his time. It is at leaſt certain, that Underwoods and Potters were found all the world over, but that they are now very generally vaniſhed ſince we have been leſs accuſtomed to look for them.

It was certainly the practice of Fraſſoni and ſome other phyſicians, who firſt uſed the bark in Italy, to adminiſter it in intermittents immediately prior to the acceſſion of the fit; and when Torti was Fraſſoni's ſcholar, in the year 1677, he aſſured his pupils, that he had never ſeen any bad effects from it, when given in this way. I imagine that Sydenham was among the firſt who introduced the practice of giving it at the termination of the paroxyſm. Dr Morton ſeems to have adopted the ſame plan, more perhaps from theory than obſervation, as there was certainly leſs hazard in giving bark prior to, or even during the fit of intermittents, than in giving it in inflammatory fevers, in which he employed it. This practice, however, has been very generally followed ſince, and even adopted by many of our preſent phyſicians.

It is now however alledged by * men of the firſt eminence in phyſic, that the bark is equally ſafe, and more effectual, when given at the acceſſion of the paroxyſm. "Opinionum commenta delet dies, naturæ judicia confirmat."

I beg leave to take notice here of an obſervation of Torti, which, I think, in ſome meaſure illuſtrates the tranſitory effects of the bark, that we ought therefore to employ it at the acceſſion of the fit. He remarks, that

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* Dr Cullen, *&c.*

that if the bark is given in the day of the leſſer paroxyſm, for example, of a double tertian, the leſſer paroxyſm is ſtopped only, leaving the fever in a ſimple tertian type; but when the bark is given in the day of the ſeverer paroxyſm, it removes both paroxyſms, and ſo as not again to return.

* Some have not only given bark at the acceſſion of the fit, but even during the exacerbation of remittent fevers, and, they alledge, with much more ſucceſs than at any other time.

† Some phyſicians have ſcrupled to give bark in the beginning of intermittent fevers; but it appears from the practice of the beſt authors ‡, that theſe fevers cannot be cured too ſoon. The jaundice and dropſies ſupervening are often the conſequence of a protracted

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* Simms, Lettſome.

† A long liſt of theſe ſuppoſed evils, from the uſe of the bark, may be ſeen in the works of Senac.

‡ Lind, Cleghorn, *&c.*

diſeaſe, and are frequently cured by bark.

From the prevailing notion entertained by phyſicians that intermittents were not to be cured but by the concoction, and more eſpecially the evacuation of a morbific matter, the aſtringency of the bark has always been advanced as an inſurmountable objection to its uſe in theſe diſeaſes. Some, however, loath to relinquiſh their ancient hypotheſes, have attempted to reconcile them with a few ſtrained facts, from which they deduce, that the operation of the bark is an evacuative power. This opinion was indeed more harmleſs than that of ſome others, who, inveighing againſt the aſtringency of the bark, conceived its operation to be analogous to the cloſe-corking of a bottle pregnant with hideous ills, ſoon to burſt forth with no leſs baneful effects than all the famed calamities of Pandora's box.

The propriety of previous evacuations

has alſo been much diſputed. I think it is now pretty generally reſolved, that, when theſe are indicated, and when we have it in our power to employ them, they certainly ought to precede the uſe of bark; but where the return of a paroxyſm might be of dubious event, or when there are manifeſt ſymptoms of great debility, evacuations ought not to take the lead of a timeous and free uſe of bark.

I apprehend it is pretty generally believed, that the bark is moſt effectual when given in ſubſtance, and in pretty large doſes.

* Many have joined to it rhubarb and other cathartics; but we preſume that theſe are for the moſt part unneceſſary, and often improper. It has been found †, that the taſte of the bark, in ſubſtance, is much covered by ſmall beer, milk, porter, extract of liquorice, or by Jamaica

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* Geoffroy, Lanciſi, Mead, *&c.*

† Lind, Home, Lewis.

ca rum *. When its uſe is accompanied with diarrhœa, it is ſometimes expedient to join to it an opiate, which is at the ſame time found to improve its effects in the cure of intermittents. Some agreeable aromatic is often proper, and frequently neceſſary to make it ſit on the more delicate ſtomachs of the ladies; and ſure no good phyſician will be ſo little of an *Aſclepiades* as to refuſe this conceſſion.

The Peruvian bark has been forbid in red urine, and during the menſes; but it has been often found † to promote a flow of urine, and bring down the menſes.

Soon after the introduction of the bark, its ſucceſs in intermittents ſuggeſted the idea that it might be uſeful in other periodical affections. Dr Morton accordingly cured himſelf of a

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* Gregory.

† Hoffman, Gregory, *&c.*

periodical hemicrania by the bark. Torti alſo tells us of an anatomiſt of the name of Pachionus, who cured a periodical ophthalmia by means of bark. And, laſtly, Dr Cullen relates a caſe of ophthalmia, which obſerved a regular tertian type, and was cured by Peruvian bark. The bark is indeed uſeful in moſt caſes of ophthalmia, whether obſerving a periodical form, or occaſioned by a laxity of the veſſels, or a ſcrophulous acrimony depoſited on the ſebaceous glands of the tarſus.

It is now, I imagine, pretty generally agreed, that the bark is the remedy we can moſt certainly rely on for the cure of intermittent fevers; but Dr Morton alſo employed it in the continued inflammatory, and indeed in all kinds of fever. I imagine this practice has been much laid aſide, till of late years the ingenious Dr Clark of Newcaſtle has again propoſed it, though under certain limitations. He is alſo followed by ſome very learned phyſicians. It does not however appear that the trials hitherto made afford any deciſive proof of the advantages of this practice; and in ſome caſes, where inflammatory diatheſis was more eſpecially marked, the uſe of the bark ſeems to have done very much harm.

In all fevers unaccompanied with a phlogiſtic diatheſis of the ſyſtem, and eſpecially when connected with much debility, the propriety of giving bark, and that too without waiting for remiſſions, is, I apprehend, generally agreed on by modern phyſicians. In that ſtate of the typhus called putrid, two ounces a day is perhaps as little as can be depended upon. In convaleſcents alſo from inflammatory fevers, the bark has been often found to be highly ſerviceable.

Bark has alſo been uſed for ſome of the *exanthemata*. Dr Monro the father raiſed and eſtabliſhed its reputation for the cure of confluent ſmall-pox. It has alſo been uſed for the miliary fever, but with what propriety is not yet agreed on by phyſicians.

It would be endleſs to enumerate all the diſeaſes in which the bark is indicated. Its effects are various, and often oppoſite * in different patients, and in different ſtates of the ſame patient. It has been generally remarked by accurate obſervers, that the bark ſtops the morbid, though ſeldom or never (if properly applied) obſtructs critical evacuations. As it is well known, that, in order to the due performance of our excretions, there is required a certain degree of tone and vigour of the ſolids; ſo we can conceive, that, in coſtiveneſs ariſing from a debility of the inteſtines, bark may be, and often is, the moſt convenient and effectual laxative. We can alſo

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* Debilioris pulſus animator, et frequentioris moderator. DE HAEN.

alſo perceive how, in a caſe of dyſentery related by Cleghorn, the bark put an end to the tertian without ſtopping the flux with which it was accompanied; and, laſtly, how the bark, in the hands of the learned Dr Wilſon of Bath, was found to be the moſt effectual remedy in the dyſentery of Newcaſtle.

From its tonic power, we may expect it to be uſeful in all diſeaſes ariſing from irritability, and whether that be or be not attended with pyrexia. Hence its utility in caſes of hyſteria unaccompanied with plethora, and in many others of the claſs of neuroſes. It has been ſuſpected * that its uſe has brought on a fit of aſthma; but I imagine that this did not proceed from the uſe of bark, any otherwiſe than by oppreſſing the debilitated ſtomach of an aſthmatic. Sir Edward Barry found the bark a very uſeful remedy for aſthmatic complaints. Every

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* Lind.

Every one knows the ſurpriſing ſucceſs of bark in the cure of gangrenes ariſing from debility, either original, or as the ſequela of inflammation. De Haen alſo found that it was very uſeful when applied externally to gangrenous ſores. It has alſo been ſucceſsfully employed in every * caſe of vitiated purulence. It may be proper to obſerve, that Mr Bell of this place is diſpoſed to think that the red bark is not ſo effectual for this purpoſe as the pale. Mr Bell's trials, however, have not been ſufficiently numerous and deciſive to induce a gentleman of his caution and ſagacity to determine abſolutely on this matter.

From the tonic powers of the bark, as diminiſhing irritability, and perhaps in ſome degree ſenſibility, it is therefore contraindicated in all diſeaſes accompanied

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* Its effects, however, on ulcers of the lungs, are more hazardous and doubtful.

nied with a morbid rigidity of the ſimple ſolids, and diminiſhed ſenſibility of the moving fibres. Hence it is often hurtful in hypochondriaſis, mania, and ſome others. As an antiſeptic, it is uſeful in all caſes of a ſeptic tendency of the fluids, not ariſing from the violence of reaction. Dr Sydenham was probably miſtaken, in accuſing it of producing ſcorbutic rheumatiſm. It is however found, that its ſucceſs in ſcurvy is not ſo remarkable as we ſhould expect from our knowledge of its effects in the fevers called putrid. This may perhaps be owing to its not entering the maſs of blood, as it appears neceſſary to the cure of ſcurvy, that our antiſeptics ſhould be alſo alimentary.

I have thus related my experiments, and freely offered my opinions on the nature and properties of the Peruvian bark.

I think it unneceſſary to make the

leaſt apology for having uſed ſo much freedom with reſpectable names.

Had I not thought them the very beſt authors on the ſubject, I ſhould neither have troubled myſelf with the ſtudy of their writings, nor the public with a view of their uſeful labours.

My attachments, on the other hand, may to ſome appear over zealous or implicit, and more eſpecially to the ſyſtem of one of the greateſt pilots that was ever at the helm of medical affairs.

From none, perhaps, ſhould I be in greater danger of incurring this cenſure, than from the very author of that ſyſtem.

Confident, however, that ſuch a character can derive no additional luſtre from my encomiums, and well knowing that my private gratitude, though great, is not to be teſtified by a ſervile ſubmiſſion to his opinions, I can aſſure my readers, that I have not adopted them becauſe they were Dr Cullen's, but becauſe I thought them agreeable to facts and ſound reaſoning.

The diſſertation, as it now ſtands, differs little from the ſtate in which it was delivered to the Harveian ſociety.

Some additional experiments on ink have been ſince inſerted; and I eſteem it my higheſt honour, that the whole is now publiſhed with the approbation of the Harveian ſociety, and more eſpecially of my learned friends Drs Duncan and Webſter.

Thoſe in the leaſt acquainted with my private concerns, will not be ſurpriſed, that in this, as in all my undertakings, I was much aſſiſted by the advice and direction of my worthy patron Mr Benjamin Bell; and I hope the public will excuſe me, that I cannot avoid ſeizing this opportunity of acknowledging my obligations to a man to whom they themſelves are ſo much indebted.

THE END.

B O O K S

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